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ABSTRACT

This report provides a comprehensive evaluation of WIN-II, the public employment and supportive service program designed to assist welfare (AFDC) recipients attain economic self support. WIN-II was fully operational from 1973-1975, and emphasized rapid job placements for employable welfare recipients. The objective of this study was to assess how successful WIN-II was in achieving its goals. WIN-II is evaluated in this report in terms of the extent to which it contributed to the following: (1) an increase in the employment (weeks worked) of participants, (2) an increase in the annual earnings, (3) a reduction in duration of welfare dependency, (4) a reduction in the amount (cost) of welfare dependency, and (5) an increase in the income of participants. The basis of this evaluation consists of several data sources, among them, the following: (1) a longitudinal series of three interviews with program participants at 78 WIN sites, (2) an identical series of interviews with a comparison group of non-participating WIN registrants, and official WIN record data on participants. In addition to estimating the net impact of the WIN-II program, i.e., the extent to which the socioeconomic status of participants changed solely as a result of WIN participation and related service receipt, the paper identifies the impact of different WIN services. The evaluation indicates that WIN-II was a heterogeneous program and that estimates of average net impact are of relatively little use. At best, they indicate that the program assisted welfare participants in achieving a slightly higher level of self support, and did so on an economically cost effective basis. (Author/AM)

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THE IMPACT OF WIN II:
A LONGITUDINAL EVALUATION

An Evaluation Submitted by:

Pacific Consultants,
together with
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Ketron, Incorporated

to:

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U.S. Department of Labor

and

Social and Rehabilitation Service
U.S. Department of Health, Education and Welfare

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U S DEPARTMENT OF HEALTH,
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Preface

This study provides an important perspective on the performance of WIN-II and the potential for welfare reform. It is based on longitudinal interviews with over 6,500 WIN participants and registrants, visits to 78 local WIN sites, extensive file searches at local WIN and welfare offices, and sophisticated computer analyses. Three consulting firms have cooperated in this effort, with Pacific Consultants as primary contractor. The report itself has been prepared by Dr. Bradley Schiller, Pacific's research director, with the assistance of Mr. David Miller and Mr. William Cameron of CAMIL Associates and Dr. Michael Temple and Mrs. Doris Hull of Ketron, Inc. The report has also benefitted substantially from extensive discussions with representatives of the U.S. Departments of Labor and HEW, particularly the Office of Program Evaluation in the U.S. Employment and Training Administration.

While we believe this report is an important contribution to manpower evaluations, we also urge readers to recognize its limits. Most importantly, the post-WIN observation period used for this evaluation, while longer than all previous efforts, is too short to yield definitive results. An additional wave of personal interviews is absolutely required if completely reliable estimates of WIN impact are to be calculated.

Philip J. Silvers
Project Director

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I. INTRODUCTION AND SUMMARY

The purpose of this report is to provide a comprehensive evaluation of WIN-II, the public employment and supportive service program designed to assist welfare (AFDC) recipients attain economic self support. Since the implementation of the first WIN projects in 1969, WIN has served over 1.2 million welfare recipients, at a cost of over one billion dollars. The program adaptation referred to as WIN-II was fully operational from 1973 to 1975, and emphasized rapid job placements for employable welfare recipients. The objective of this study is to assess how successful WIN-II was in achieving its goals. In accordance with those goals, WIN-II has been with evaluated in terms of the extent to which it contributed to:

- an increase in the employment (weeks worked) of participants;
- an increase in the annual earnings of participants;
- a reduction in the duration of welfare dependency;
- a reduction in the amount (cost) of welfare dependency; and
- an increase in the income of participants.

A. The Evaluation Effort

The basis for this evaluation consists of several data sources, namely: (1) a longitudinal series of three interviews with program participants at 78 WIN sites; (2) an identical series of interviews with a comparison group of non-participating WIN registrants; (3) official WIN record data on participants; (4) welfare office record data on both participants and the comparison group; (5) on-site assessments of program structure and performance; and (6) supplemental data collection. The first wave of interviews began in

March 1974, and included 3,084 participants and 3,471 individuals in the designated comparison group. The third wave of interviews was conducted during the period May-September 1975 and included 5,298 individuals or 81 percent of the interview sample. Record data was collected on all of the sampled participants, from both WIN and welfare office files, while on-site assessments of program structure and performance, at all 78 sites, were conducted.

The focus of our analysis is on the "net" impact of WIN; i.e., the extent to which the socioeconomic status of participants changed solely as a result of WIN participation and related service receipt. In gauging net impact we compare the observed status gains of WIN participants to those of a comparable group of welfare recipients who did not participate in the program; the difference in status gains is the basic measure of net gain attributible to WIN services. The entire analysis is carried out in a multivariate framework, of course, taking into account the influence of demographic characteristics and labor market conditions.

In addition to estimating the net impact of the WIN-II program, we have also attempted to identify the impact of different WIN services. The extent and kind of services received by any participant depends not only on his or her needs and potential, but also on the resources available at that particular site and the efficiency with which they are mobilized. Accordingly, it is reasonable to anticipate that the net impact of WIN will vary widely across individuals and local programs. By analyzing such variation we can provide useful information for program design and administration, thus ultimately enhancing average net impact.

B. Major Findings

The details of our analysis as well as descriptive summaries of WIN structure and performance are contained in subsequent chapters. As a prelude to those discussions, we here provide a summary of our findings, organized around five major subjects, namely: WIN Program Structure; WIN Client Profiles; Average Net Impact; Impact of Specific Services; and Cost Effectiveness. We also provide a brief statement of general policy implications. For convenience, we have indicated page references to further discussions in the body of the report. Our major findings follow:

B.1 WIN Program Structure:

- As implemented at local sites, WIN-II was an extremely heterogeneous program. The evolution of WIN, through WIN-I (1968), WIN-II (1972), and the "WIN Redesign" (1975), together with varying speeds of local implementation of each stage, served to create a "program" with tremendous local diversity. A national WIN-II program has existed only in terms of the kinds of services potentially available at local sites and the kinds of welfare (AFDC) recipients likely to be served. (pp. 11-16)

- The exposure of AFDC recipients to employment services was accelerated. Under WIN-II the Employment Service (ES) assumed an increasingly active role in the registration of WIN-II participants, thereby exposing clients to ES much earlier in their welfare experience. This trend, which anticipated the later WIN Redesign was intended to reduce the welfare inflow (the "deterrent effect"). (pp. 16-17)

- Direct placement activities were given increased emphasis at nearly all WIN sites. Despite the substantial variation across local sites in structure, staffing, and service provision, certain trends were apparent. Most important was the increasing focus on direct placement, as reflected in reduced availability of nonplacement services and a greater tendency to service the most employable applicants. (pp. 17-20)

- The provision of supportive services declined. WIN's increasing focus on job-ready applicants reduced the need for supportive services. At the same time, local sites demonstrated an increasing reluctance to provide supportive services (especially child care) for participants in orientation, job search, education, or training components. (pp. 20-21)

- WIN has become increasingly dependent on CETA for training, OJT, and PSE resources. Cutbacks in WIN resources led WIN projects to depend heavily on CETA for training and PSE services. By Spring 1975, CETA was providing between one-third and one-half of the institutional training available to WIN participants and similarly high proportions of OJT and PSE. (pp. 22-24)

B.2 WIN Client Profiles:

- WIN-II registrants had less welfare experience and more employment experience than the typical adult AFDC recipient. A combination of mandatory exemption criteria and an increasing tendency to focus registration efforts on new AFDC applicants served to distinguish WIN registrants from the larger welfare population: nearly 40 percent of WIN-II registrants had welfare case openings and employment experience in the most recent six pre-WIN months. By contrast, less than 18 percent of all AFDC recipients had been on welfare less than six months, and only 30 percent have recent work experience. (pp. 26-28)

- WIN-II participants tended to be more employable than registrants. In conformity with program guidelines, WIN-II gave priority to the most job-ready registrants. Over 50 percent of the WIN-II participants had been on welfare less than six months (vs. 40 percent for all registrants), and were more likely to be male (30 vs. 24 percent) and high school graduates (45 vs. 36 percent). (p. 28 & 30)

- Approximately one out of three WIN-II participants were reported as later employed, and one out of nine were observed to have left welfare. Of the roughly half million participants annually enrolled in WIN, 175,000 (34%) found jobs in each of fiscal years 1974 and 1975. Approximately two thirds of these jobs lasted ninety days or longer (Completed Job Entry). Only 52,000 WIN participants were reported as having left welfare. (p. 33)

- An increasing percentage of total WIN employment has been in OJT or PSE. The percentage of OJT slots in total WIN employment grew from 18% in FY 73 to 22% in FY 75, while the percentage of PSE slots grew from 3% to 8%. These trends reflect the component emphasis mandated by the Talmadge Amendments. (p. 35)

B.3 Average Net Impact of WIN:

- The average net annual earnings gain experienced by male participants was in the range $330-$445 and by females in the range $327-$471. That is to say, the earnings gains experienced by WIN participants in the first year after participation were larger than the gains experienced by comparable, but nonparticipating WIN registrants. (pp. 61-64)

- The average net gain in annual employment was 2-3 weeks for males, 3-4 weeks for females. (pp. 60-61)

- Virtually no net reduction in welfare dependency occured, on average. No net reduction in the average time on welfare was observed, for male or female participants. Only small reductions ($164 per year for males, $106 for females) in the average welfare grant occurred. This means that WIN participants were no more likely, on average, to leave welfare than non-participating registrants with similar characteristics. (pp. 60-62)

- The net income gain to WIN-II participants averaged $561 per year for females, $290 for males. Analysis of a subgroup of all WIN-II participants, on whom complete income data was available, indicates that welfare regulations permit female participants to retain most of their earnings gains, without offsetting grant reductions. For males, the implicit "welfare tax" is over 40 percent. (pp. 70-73)

- WIN-II had much more success, on a net basis, with participants who were less job-ready. The average net impact of WIN-II reflects the experiences of two very different kinds of clients. Participants who entered WIN with little or no recent employment experience achieved very substantial first-year net earnings gains ($604 for males, $676 for females), while participants with recent work experience attained little ($190 for males, $41 for females). (pp. 125-132)

- The average net benefit to black WIN participants is far below the average net benefit to whites. The average net earnings gains for black males was zero or negative, while for white males it was $580. Similarly, the average net earnings gain in the first year for black females was $255, compared to $634 for white females. (pp. 71-73)

- The 1973-75 recession constrained WIN's performance. Comparisons of participant experiences in areas where the unemployment rate increased substantially to those in less adverse labor markets suggest that the average net impact of WIN-II was significantly constrained by deteriorating labor market conditions. Thus, our impact estimates do not fully express WIN's potential in an improved labor-market setting. (pp. 81-93)

B.4 The Impact of Specific Services:

- The level of employment services provided to individual participants varied tremendously. Seven percent of WIN "participants" received no employment services, while others received widely varying levels of education, vocational training, placement, or OJT/PSE. (PP. 95-114)

- The net impact of specific services varied greatly, with higher levels of service yielding the greatest net impact. Nonserved ("unassigned") participants actually experienced net status losses, when compared with similar WIN registrants. Placement services yielded a net first-year gain of $231-$361 for females and no net gains for males. By contrast, institutional training generated a net gain of $469-$622 for females and up to $722 for males, while subsidized employment (OJT and PSE) generated net first-year gains of approximately $1,500 for females and $1,900 for males.* (pp. 121-124)

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* First-year earnings gains attributible to OJT and PSE are publicly subsidized.

- Net earnings gains to black participants were smaller at every WIN service level. Black males actually manifest no net earnings gains from WIN services other than OJT/PSE; and even at that level of service the net first-year earnings gains to black males is only one-third that attained by white males. Racial differences are not so large for females. (pp. 124-126)

- Prior work experience appears to have been the key determinant of net benefit, with the least experienced receiving the greatest net benefits. Lower-level WIN services (e.g., placement) yielded no net earnings gains to males or females with recent employment experience, but some gain to those without recent experience. Less experienced participants gained substantially ($840 for females, $1,346 for males) from institutional training, while more experienced participants gained little. The net earnings gains from OJT/PSE were likewise much larger for less experienced participants. (pp. 126-132)

- The WIN tax credit was not effective in stimulating employment. Little use was made of the tax credit, due to a high percentage of self-placements its restrictive provisions, and a reluctance to identify welfare recipients in employment settings. (pp. 107-108)

B.5 Cost Effectiveness:

- The economic benefits of WIN-II exceeded its costs. The present value of net earnings gains to both males and females were greater than their associated employment and supportive-service costs. In this sense WIN-II was economically cost effective. (pp. 161-169)

- The welfare savings attributable to WIN-II were smaller than direct program costs. Work incentive provisions built into the AFDC grant structure preclude welfare savings equal to earnings gains. As a consequence, average welfare savings ($400 for males, $259 for females) were considerably less than average program costs ($760 for males, $1,104 for females). (pp. 161-169)

- Institutional Training and OJT/PSE were more cost effective than placement services. Although training and subsidized employment activities were much more expensive than placement activity, they yielded still higher levels of benefit. Institutional training yielded the highest ratios of net earnings gain to cost. (pp. 171-175)

- All WIN-II services attained greater cost effectiveness in serving the less job-ready. The higher costs of serving participants with no recent work experience are more than compensated by resultant net earnings gains, while services rendered to more experienced participants rarely yield comparable net gains. (pp. 176-179)

C. General Policy Implications

A salient impression yielded by our evaluation is that WIN-II was a very heterogeneous program and that estimates of average net impact are of relatively little use. At best, they simply indicate that the national WIN-II effort did in fact assist welfare participants achieve a slightly higher level of self-support, and did so on an economically cost effective basis. Much more interesting than WIN's average net impact is the observed variation in impact associated with specific WIN services and client profiles, for such variation provides general guidelines for program and policy planning. On the basis of this variation, the following conclusions seem warranted:

- The participation priority accorded to job-ready applicants was ill-advised and should be abandoned. AFDC recipients with good work experience receive little or no net benefit from WIN, as they are just as likely to find employment and leave welfare on their own. On the other hand, those recipients with little work experience benefit substantially from WIN services. (pp. 181-182)
- The "revolving door" concept of program service has yielded little benefit and should be sacrificed to greater service provision. The emphasis on direct placement of job-ready clients has absorbed much of WIN's attention, yet yielded no demonstrable net benefit. On the other hand, training and OJT/PSE services yield net status improvements that exceed their costs. Accordingly, the revolving door concept can be justified only if a substantial "deterrent effect" can be documented. This has not been done, and available evidence suggests that the deterrent effect may be insignificant. (pp. 186-187)

- Gross placement rates, CJE rates, and welfare reduction rates are misleading indices of WIN impact and should be de-emphasized. Gross-flow indices of performance are not correlated with net impact. This discrepancy is explained by the fact that job-ready clients readily boost placement rates but do not achieve net status improvements. (pp. 38-41, 125-132)

- WIN-II's emphasis on OJT/PSE should continue. The observed net benefit to participants enrolled in OJT/PSE far exceeds that yielded by any other WIN component and appears to be cost effective. While these observed benefits include public subsidies, there is no evidence yet that net benefits will fall precipitously after OJT/PSE subsidies end. Until more evidence is available on this critical question, continued OJT/PSE emphasis seems warranted. (pp. 185-186)

- An inquiry into why black participants benefit less from WIN services should be undertaken. Under WIN-II black participants received less benefit at all service levels. Whether this discrepancy is attributible to labor market discrimination alone or to program resource allocations is not known.

- Further efforts should be undertaken to assemble longitudinal evaluation data bases. A major constraint on the present evaluation effort is the lack of follow-up data on WIN participants who were enrolled in OJT/PSE; this constraint should be eliminated with an additional wave of interviews. Beyond this, possibilities for continuing longitudinal data bases should be explored further.

A more complete discussion of these and other policy implications is provided in Chapter IX, together with some consideration of important policy constraints.

II. THE WIN PROGRAM: AN OVERVIEW

Since its introduction in 1967 the WIN program has been the most visible manifestation of public efforts to move welfare recipients from a condition of public dependency to one of financial independence. In FY 1969, its first operational year, WIN provided 218,000 adult welfare recipients with educational, vocational, or other employment services. By the end of FY 1975, WIN had served over 1.2 million recipients, and was actively providing some form of employment assistance to 200,000 participants. While the effort might appear modest in view of the fact that an average of four to five million adults has received welfare (Aid to Families with Dependent Children) each year during this period,* WIN's effort has been impressive and growing.

What makes WIN even more interesting is the fact that its growth has taken place during a period of vigorous national debate on welfare reform. That debate was initiated in August, 1969 when President Nixon called for a major overhaul of the nation's welfare system, one which would transform "welfare" into "workfare". The debate was maintained throughout the period 1969-71, as Congress deliberated over the specifics of the President's proposed reforms (the Family Assistance Plan) and has continued right up until 1976, with renewed calls for welfare reform being issued by both Presidential candidates. During all this time, WIN has continued to grow, largely unnoticed. Yet, the WIN program has incorporated at one time or another just about every mechanism yet suggested for facilitating the movement from welfare to workfare, including child care assistance, literacy training, basic education, vocational training, subsidized on-the-job training, training stipends, job referral service, personal and vocational counseling, transportation assistance and subsidies, medical examinations, partial, even fully subsidized employment, and improved incentive structures for recipients.

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* An average of three million adults was receiving AFDC at any time. As a consequence of turnover in the welfare caseload, the number of adults receiving welfare at some (any) time in the year is much larger than the average caseload. See Bradley Schiller, "Turnover Rates in Public Assistance Programs," Welfare in Review September, 1970 or Ketron, Inc. "Estimates of Annual Natural Turnover Rates from the 1969 and 1971 AFDC National Surveys," November, 1972.

In view of both its size and its resemblance to major features of welfare reform, WIN's performance merits careful examination and evaluation. We begin such an effort in this chapter by reviewing the major features and dimensions of WIN, then proceed in subsequent chapters to an evaluation of the program's impact on welfare recipients.

A. Evolution of WIN-II, 1968-73

The WIN program is probably best viewed as a logical extension of the earlier, more broadly-targeted employment and training programs introduced by the federal government in the early 1960's (e.g., MDTA). While those programs provided many of the same services later to be offered by WIN, they were not explicitly designed to meet the needs of the very poor, much less those who depended on public assistance. As they grew, increased attention was directed to this omission and various strategies were attempted for reducing "hard-core" unemployment and associated welfare dependency. Various degrees of "outreach" by established programs were not notably successful, however, in substantially reducing welfare dependency, even in the context of falling unemployment rates. As a result, more explicit efforts to serve the welfare population were introduced.

The 1967 Amendments to the Social Security Act (SSA) introduced the first truly national program (now referred to as the WIN-I program) to promote the self-support of welfare recipients. The basic idea behind the WIN-I program was to increase the employability of welfare recipients via a combination of counseling, orientation to the world of work, education, and vocational training. It was assumed, of course, that such enhanced employability would increase the movement from welfare to workfare, and thus offer a long-term solution to the nation's welfare program. To help assure that the program would achieve this objective, it was required of the states, called for explicit interaction between the Departments of Labor and HEW, and was mandatory for certain categories of AFDC recipients.*

While the WIN-I program enrolled over 700,000 welfare recipients in its first four years of operation, its success was constrained by several factors. First, the economy experienced a recession beginning in late 1969, thus making jobs and on-the-job training slots more difficult to attain. Second, there was not much experience with employability programs for the hardest-to-employ, particularly welfare mothers. Third, the WIN-I program called for a higher level of interaction between the Departments of Labor and the HEW than had previously existed, and this resulted in many unanticipated start-up costs. On the Labor side, there was little experience, particularly at the Employment Service (ES), in dealing with welfare clients, and some suspicion that local welfare departments were not cooperating fully in the referral process. On the HEW side, there was some resentment about the loss of responsibility for welfare recipients, and a sense that ES was insensitive to welfare client needs. Finally, in 1969, President Nixon proposed sweeping welfare reform measures (the Family Assistance Plan), which diverted managerial attention away from WIN.**

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* The degree to which WIN-I was actually mandatory varied considerably from place to place because the Department of Welfare referred suitable individuals to the ES/WIN units. Referral varied from nearly everyone receiving AFDC to practically no one. This was, as will be discussed, one of the major reasons for the 1971 Amendments to the SSA which authorized what became known as the WIN-II program.

As a result of these many start-up difficulties, plus increasing national frustration with seemingly irreducible welfare caseloads, the WIN program was subjected to widespread criticism and restructuring shortly after it became fully operational. Most important in this regard were the 1971 Talmadge Amendments to the Social Security Act, which created the program structure referred to as WIN-II. What distinguished WIN-II from its predecessor was its emphasis on quick placement rather than employability development, mandatory participation for all welfare adults (unless specifically exempted), and better coordination between the Departments of Labor and HEW. The administrative requirements for implementing these structural shifts included:

° Replacement of the referral process, which was highly variable, by a registration process, required of all recipients except for those specifically exempted.

** Although DHEW had several hundred persons working on the implementation of FAP, not one individual had WIN as his full-time job in either the national or regional offices. The ratio in DOL, although not quite as extreme, was similarly imbalanced.

- Institution of joint DHEW-DOL coordination at all levels including the joint issuances of guidelines, directives, etc.
- Preparation of local and statewide operational plans to provide for local planning and for federal review.
- Implementation of Separate Administrative Units (SAUs) within each Department of Welfare to provide supportive services in support of WIN and to administer DHEW's responsibility at the state and local levels.
- Emphasis on On-the-Job training and Public Service Employment by specifying that one-third of all expenditures be for these components.
- Establishment of Local labor market advisory councils.
- Mandatory certification to the Secretary of Labor of 15 percent of all persons required to be registered in each year.
- Tax credits for wages and other expenses for employers hiring WIN participants (included in the Revenue Act of 1971, Title VI).

Although the Talmadge Amendments called for the implementation of WIN-II on July 1, 1972, local sites were unable to complete all of the required changes in so short a period. While it is convenient to view the structural changes mandated by the Talmadge Amendments as logical extensions of the earlier WIN-I effort, they turned out to be more revolutionary than evolutionary, the main reason being the consolidation of almost all WIN activity into the ES structure. Largely as a result, WIN-II was implemented with varying speed, and cannot be regarded as fully operational until early 1973.

The immediate intent of the 1971 changes had been to correct deficiencies in WIN, particularly in the irregular referral process and in the limited availability of placement-related components such as OJT. The amendments were seen, however, as a step toward welfare reform which would have assigned responsib for all employable recipients to the Department of Labor. As a result of this view of the program, a WIN intake structure was created which resulted in having WIN registration forms prepared and sent to the ES/WIN office. Selection for call-up was made from these forms (the registrant pool) by ES/WIN staff who also controlled the decision to certify and the specifications of certification services. SAU staff would, however, usually be involved in the appraisal itself.

The effort to have the entire intake procedure controlled by the ES/WIN component produced a very cumbersome intake procedure which resulted in long delays between registration and appraisal. During the first year of WIN-II, the delays were even greater because of the enormous workload created by the mandatory registration of the entire non-exempt AFDC caseload between July and December, 1972, a process which actually yielded 1.2 million registrants in the first year (FY 1973).

During this first year of WIN-II, major changes were also taking place in the basic orientation of the program and in the relationship between the two agencies. WIN-I, as mentioned earlier, was designed largely as an employability development program stressing institutional training, education, work experience, and other in-house developmental components. Under WIN-II, increasing emphasis was placed on placement, and in particular, on direct placement, i.e., placement not involving services. This resulted not only in changes in the basic approach to the WIN recipients' employability, but also in changes in the type of recipient selected for participation and in the staffing of WIN itself.

On the HEW side, the creation of the Separate Administrative Units produced major changes in the provision of service and in the way registrations were conducted. Under WIN-I, it was typical for each caseworker* to review the current caseload, assess the suitability for WIN, determine the need for service, and refer those selected to WIN. Under WIN-II, registration became a simple function of the income maintenance worker. The SAU staff usually came in contact with the recipient only at appraisal. The staff's role was then often restricted to providing services approved by the ES/WIN staff. Because of this, SAU staff became increasingly oriented to the WIN activity itself, and increasingly integrated into the WIN program, both physically and functionally.

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* The change from WIN-I to WIN-II also coincided with the shift to separated welfare departments, i.e., an income-maintenance section to handle grants, and a service section to handle services. Under WIN-II, registration (until regulations published in 1975) was conducted by the IMU and service provision by the SAU staff within the services section.

B. Program Changes, 1974-75

The foregoing section provided a quick review of the background of WIN-II. We must recognize, however, that the evolutionary development of WIN did not cease with the full implementation of WIN-II. On the contrary, it is fair to say that the WIN program has yet to achieve a steady state. Not only did WIN-II continue to evolve in a variety of directions after 1973, but in addition a major structural change was introduced in September, 1975 (the "WIN redesign")*. While we are not interested here in all the details of this evolution, we may review the more significant trends underway during the evaluation period (Spring 1974 - Fall 1975), as they may have influenced program outcomes. The most noteworthy trends in this regard were: increased ES involvement in the registration process; increased emphasis on "direct placement"; decreased provision of supportive services; and increased use of CETA resources. Each of these changes is discussed briefly in the following paragraphs.

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* See the Federal Register, vol. 40, p. 43170, no. 182, Sept. 18, 1975. The effects of the WIN Redesign actually predate its implementation, as administrative reforms had been suggested a year earlier and partially implemented at many sites.

B. 1 Increased ES Role in Registration:

In the first area — registration — there was an increasing tendency for registration of AFDC applicants to be performed directly by ES/WIN, instead of by the IMU. The original WIN-II registration process, which was used in virtually all sites in 1974, relied on the IMU to register WIN applicants, then forward their signed forms to the ES/WIN office for appraisal scheduling. By Spring, 1975, nearly one-third of the WIN sites had altered this process by referring applicants directly to ES/WIN for registration, thus allowing ES/WIN to obtain an immediate work history from applicants.

The long-run significance of this change lies in its faster exposure of AFDC applicants to manpower services. To the extent that this exposure is effective, it should slow the rate of welfare inflow, (the "deterrent effect"), in addition to any effect it has in hastening movement off welfare (the more conventional welfare to workfare image)* The short-run significance of the change may lie in the implied shift of manpower resources from "old" to "new" WIN registrants and resultant reduction in placements of the study population. In this regard, it is important to note that only one out of five sites was performing reappraisal of persons in the registrant pool in 1975, as contrasted with better than one out of four in 1974. The remaining sites relied exclusively on new entrants to welfare as the source on WIN participants.

B. 2 Increasing Focus on Direct Placement:

The increasing tendency of WIN to focus exclusively on placement activity was evident during the study period. While the percentage of sites offering nothing more than placement remained the same (32%) between Spring, 1974 and Fall, 1975, most other sites cut back dramatically on their use of nonplacement services.

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* The deterrent effect was the avowed intention of the WIN Redesign, which made this registration change mandatory, effective September, 1975. Our field observations confirmed that many sites had "jumped the gun" on the proposed guideline changes.

As pointed out earlier, WIN originated as a training program. Early WIN-I participants frequently received a rather leisurely sequence of components, including program orientation, educational components (basic education, high school equivalency,\ special courses to prepare for civil service examinations, English as a second language), training (institutional training, work experience, on-the-job training), vocational counseling, job referral, and follow-up. There was a wide range of "in-house" services, and considerable use of purchased institutional training. Some WIN participants even went to college at the program's expense. While really all of these are theoretically still available,* their use has steadily given way to concentration on direct placement of participants. In many areas, WIN has become, in effect, a separate labor exchange for welfare recipients , a phenomenon resembling what many call the "revolving door" concept of program service.

A measure of the increasing emphasis on placement activity is provided in Table 2.1. In this table we have indicated the number of sites offering various WIN services, first in the Spring of 1974, then again in the Spring of 1975. Most noteworthy are the reductions in the number of sites offering orientation, institutional training, work experience, OJT, and PSE. What this indicates, of course, is a continuing move away from the employability

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* Additionally, WIN now has public service employment (PSE) and job search components, as well as a "suspense" status which permits a participant to enroll in other manpower programs such as CETA. The practice of sending participants to college has been discontinued.

TABLE 2.1:

Changes in Service Availability, 1974-75 *

	Percents Of Sites Offering	
Service Provided	1974	1975
Orientation either an explanation of the WIN program itself, including participants' legal rights and obligations or an explanation of the "world of work"	62%	51%
Counseling primarily vocational, but also personal	65%	73%
Institutional Training classroom vocational training	69%	49%
Work Experience limited work in the WIN office or some other public or nonprofit institution, usually for a five-week period	53%	41%
On-the-Job Training subsidized (50%) employment in a private sector job with explicit training provisions	56%	29%
Public Service Employment subsidized (100%) employment in a public or nonprofit institution, typically for one year	51%	37%
Classroom Education	60%	53%
Adult Basic Education (ABE)	59%	67%
General Educational Development (GED)	26%	31%
English as a Second Language (ESL)		
Job Search review of job listings, often on a regular, mandatory schedule	28%	51%
Placement	100%	100%

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* Based on a sample of 78 WIN sites

development concept associated with WIN I. The other side of this coin is reflected in the increased availability of vocational counseling, job search assistance, and placement activities.

The reduction in non-placement services, together with expanded emphasis on placement, while consistent with legislative mandates, has served to limit access to the program. In particular, there is an increasing tendency to reserve scarce program resources for the most employable applicants. In 1974, about two-thirds of the sites were engaged in selective screening; by 1975, virtually all were. This "creaming" as it is often called, varied; in its most severe manifestations, it involved eliminating from consideration for the program those with characteristics that marked them as hard to place: those with less than an eighth grade education, those with more than four children, those on welfare for long periods, those considered too old, and others deemed unattractive to prospective employers (e.g., obese, disfigured, speech problems). The result, of course, is a program that serves those who would be likeliest-to-succeed, on their own, and an attendant increase in program "success" rates.*

B. 3 Declining Provision of Supportive Services:

Proceeding in parallel fashion with the increasing emphasis on placement activities has been a decreasing provision of supportive services. Although the Separative Administrative Units (SAUs) charged with identifying and meeting clients' child care, transportation, and other support-service needs have become fully integrated into the WIN structure, a trend toward reduced service provision is apparent. With the increased emphasis on quick placement and the associated tendency to "cream" the registrant pool, those registrants with severe service needs are often bypassed in favor of the more readily employable. This means that the role of services — like the role of employability development components — has diminished.

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* Our evaluation methodology — in contrast to raw placement statistics commonly reported by program administrators — explicitly controls for demographic characteristics, and thus reduces somewhat the effects of "screening" bias.

Not surprisingly, the WIN activities for which child care would be provided were more restricted in 1975 than in the previous year. In 1974, one-third of the sites permitted child care to be provided during any WIN activity; by 1975, the percentage had declined to less than one-fifth. At the same time, the percentage of WIN sites providing child care exclusively for employment increased from thirty-one to thirty-eight, with an additional third of the sites extending child care to training components. It was increasingly rare to find child care available for participants in orientation, job search, education components, or work experience.

As with child care, there are increasing restrictions on which components can entail transportation assistance. Only 44 percent of the sites provided transportation assistance through all WIN activities in 1975, down from 51 percent in 1974. At other sites, assistance was limited to training, job search, and job entry components, in various combinations.*

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* There was no apparent pattern to the combinations. One site might offer transportation assistance for training components only, another for training plus placement, another for placement only, and so on.

B. 4 Increasing Use of CETA:

The availability of services provided by the Comprehensive Employment and Training Act (CETA) of 1973 has been a factor of increasing significance of WIN. Primse sponsors of CETA have assumed most of the local burden for all public manpower services, and in this capacity have become a substantial source of training resources for WIN. The availability of CETA services has been particularly importa as WIN budgets have been cut back and purchases of training, OJT and PSE have been restricted.

In 1975, 50 percent of the WIN sites had some involvement with CETA compared with only 14 percent in 1974. The relationship varied from sites in which WIN and CETA were fully integrated, to those which relied on CETA for training and subsidized employment, to those which simply purchased a minor service (e.g., orientation) from CETA. The most common relationship between WIN and CETA was in the provision of institutional training. Of those sites providing more than token training (4 slots or more), over half relied on a CETA for all their institutional training, while an additional third had some participants enrolled in CETA-funded programs.* CETA was also involved in the provision of other components. At a few sites, more than one-third of all OJT came from CETA, and at one out of three other sites with OJT, at least a few WIN participants were in CETA-funded OJT slots. In addition, at two-thirds of the sites offering PSE opportunities, some WIN participants were in CETA-funded and developed PSE positions (See Table 2.2).

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* This did not mean, however, that all other training was funded through WIN. With the budget cuts, WIN relied on Basic Education Opportunity grants, state scholarships, and other methods not requiring the use of direct WIN resources.

Table 2.2:

WIN UTILIZATION OF CETA, SPRING 1975

Extent of CETA Utilization	Institutional Training	OJT	PSE
None	3 sites	33 sites	10 sites
Some (less than one-third of slots)	10 sites	14 sites	19 sites
Substantial	25 sites	3 sites	
Total*	38 sites	50 sites	29 sites

* includes only those WIN sites having at least three slots in training, OJT, or PSE

The increase in the use of CETA-funded programs is only the beginning of a trend, according to WIN managers. With their own funds reduced for components, CETA has become the only alternative. To the extent that WIN participants are increasingly found in CETA programs, the real cost of WIN may become considerably more than that reported on WIN expenditure summaries and the effect of reductions in WIN funds may simply be to "obligate" expenditures in other programs.

C. The Client Population

As noted earlier, WIN is intended to serve the AFDC population. However, not all adult AFDC recipients participated in, or even registered for WIN; Of the more than four million individuals aged 16 or over who received AFDC benefit in FY 1973, 1.2 million were registered and only 353,900 actually participated in the WIN program. The tremendous disparities in these numbers reflect a combination of statutory exemptions and budget constraints.

C.1 The Exemption Process:

The primary basis for the disparity in the sizes of the welfare and WIN registrant populations lies in the exemption provisions of WIN's enabling legislation. According to the Social Security Amendments of 1971, all adult AFDC recipients are to be registered for WIN services unless specifically exempted. Those exempted include:

- children under the age of sixteen
- children between the age of sixteen and twenty-one attending school full-time
- individuals who are ill
- individuals with physical or mental impairments
- adults aged 65 or older
- individuals living outside a WIN area
- adults who are needed as caretakers in the home
- mothers of children under six years old
- mothers of children in households where the nonexempt father participants in the program

In practice the determination of exemption is straightforward, and happens in much the same way at all sites. There is some variation across sites, however, in the determination of medical and remoteness exemptions. In some states an applicant's unsupported claim of physical handicap is accepted as the basis for exemption. In most sites, though, the applicant's claim has to be supported with medical corroboration, frequently including a physical examination. With respect to the "remoteness," some rural sites made considerable use of this exemption, while others had policies that sharply restricted, or even precluded, its use. One site, for example, refused to exempt male youths for remoteness, suggesting to them that they move closer to the site. At other sites, all applicants, regardless of remoteness, had to participate at least to the point of appraisal. Typically, when they were appraised they were not certified because of unmeetable service (transportation) needs. In general, there was a discernible toughening in the area of exemptions, particularly medical ones, with the avowed intent of increasing the exposure of AFDC recipients to WIN.

As Table 2.3 suggests, 89 percent of all exemptee are women, and most are mothers with children under the age of six; for these mothers no verification beyond that required for welfare is generally requested. When a recipient has been determined exempt, regulations call for a redetermination every six months. In practice, sites vary greatly in their adherence to this regulation. Many sites, particularly smaller ones, simply redetermine when the original reason for exemption has apparently changed (temporarily disability corrected,

Table 2.3 Reasons for WIN Exemption, by Race

EXEMPTEE

REASON	MALE	FEMALE
Under 16	0 %	0.8%
In School	16.9%	0.9%
Ill or incapacitated	66.1%	14.0%
Remoteness	3.4%	2.3%
Family Illness	6.8%	3.3%
Children under six	6.8%	73.8%
Care at home	0.0%	2.8%
Combination	0.0%	1.8%
Total	100 %	100 %
Column Total as Percent of All Exemptions	11 %	89 %

youngest child passes six years of age). In large sites, exemption from WIN may never be reconsidered after the initial determination.*

C. 2 The AFDC vs. WIN Registrant Populations:

The exemption process not only significantly reduces the number of individuals potentially eligible for WIN services, but also makes the registrant pool unrepresentative of the entire adult AFDC population. Some sense of the differences between the AFDC and WIN registrant populations can be obtained from Table 2.4 which contrasts the demographic characteristics of the AFDC, WIN registrant, and WIN participant (adult) populations.** Predictably, the WIN registrant pool contains a much higher percentage of males than the underlying AFDC population, has a significantly larger proportion of whites, and a greater percentage of high school graduates.

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* This helps explain why many exemptees later find jobs and move off welfare, a phenomenon discussed in Appendix E.

** These comparisons must be qualified, as the two data sources are not perfectly compatible. Statistics for the AFDC population are taken from the Office of Information Sciences, Findings of the AFDC Study, Part 1, "Demographic and Program Characteristics" (U.S. Department of Health, Education and Welfare, Social Rehabilitation Service), and refer to mothers and fathers, while data on the registrant pool refer to FY 1974 and are based on a random cross-sectional sampling of registrants at the 78 sites used for this study. The participant profile is based on our study sample of "new" participants in Spring 1974.

Table 2.4:

SUMMARY TABLE OF DEMOGRAPHIC CHARACTERISTICS FOR THE AFDC, WIN REGISTRANT AND WIN PARTICIPANT POPULATIONS

		1973 AFDC Population*	1974 WIN Registrants	1974 WIN Participants
SEX:	Male	11.9%	24.3%	30.0%
	Female	88.1%	75.7%	70.0%
AGE:	Under 25	29.9%	27.9%	43.5%
	Over 25	70.1%	72.1%	56.5%
RACE:	White	46.9%	54.1%	51.1%
	Minority	53.1%	45.9%	48.9%
EDUCATION:				
	Less than 12th grade	69.0%	64.2%	55.2%
	12th grade or more	31.0%	35.8%	44.8%
FAMILY SIZE:				
	2 or less	34.3%	26.2%	27.6%
	3-4	40.2%	42.8%	47.0%
	5 or more	25.5%	31.0%	25.5%
MONTHS ON WELFARE:				
	1-6	17.4%	40.6%	50.3%
	7-12	12.9%	10.2%	9.4%
	13-24	19.2%	13.1%	12.7%
	25 or more	50.4%	36.0%	27.6%
MONTHS SINCE LAST FULL-TIME JOB:				
	Never worked	25.6%	19.3%	16.8%
	Now working	20.4%	18.1%	7.7%
	1-6	9.1%	21.0%	31.9%
	7-12	6.9%	8.5%	12.8%
	13-24	9.3%	8.9%	11.1%
	25 or more	28.7%	24.2%	19.6%

* Includes all natural and adoptive parents included in AFDC household.

It was expected that these differences between the WIN registrant and AFDC populations would increase as the 1973-75 recession worsened and forced more families out of "workfare" and onto "welfare". Those individuals most recently employed would be expected, of course, to possess the greatest employability. According to the WIN staff at most sites, however, this trend did not appear during the evaluation period, as extensions of unemployment insurance coverage and duration kep many employables, particularly males with good work histories, off of welfare. Thus, the anticipated influx of job-ready registrants did not materialize.*

The most noteworthy differences between the AFDC and WIN Registrant samples are in their welfare and employment histories. While only 17.4 percent of the AFDC population has come onto welfare within the last six months, over 40 percent of the WIN registrants are recent entrants to the welfare rolls. This disparity clearly affirms our earlier suggestion (in section IIB) that WIN is focusing on new AFDC applicants, thus seeking to stem the welfare inflow. Further evidence of this trend is apparent in the employment histories of the two groups, which indicates that WIN registrants tend to have very recent employment experience. For many of them, their application to welfare appears to have been occasioned by a recent job loss.

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* When the benefits are exhausted, however, the characteristics of the registrant pool may change. This did not occur to any great extent during the study period. See Chapter V for a fuller discussion of the impact of recession on client characteristics and outcomes.

C. 3 The Registrant vs. Participant Populations:

The limited resources of WIN effectively preclude 100 percent participation by the registrant pool. As a result, actual participation depends on the availability of services at a local site and the selection criteria imposed to restrict access to those services. As noted earlier, these criteria tend to reflect employability potential.

The statistical differences between the registrant pool and the participant population are also depicted in Table 2.4. Clearly, there is a greater tendency for participants to be male and to have completed high school. Individually and in combination these characteristics tend to be associated with higher rates of employment, and thus operate as effective "creaming" criteria. This, of course, is an explicit objective of the program, as noted earlier.

It is again important to note the different welfare and employment histories of the WIN participant group: over half are very recent entrants to welfare or new applicants, while nearly one-third have lost their jobs within the last six months. Thus, the participant group further reflects the tendency, already noted in the registrant pool, for WIN to concentrate its resources on stemming the welfare inflow.

D. Operational Performance

The ultimate objective of the WIN program is to reduce the size of the nation's welfare rolls, to move people from welfare to workfare. The early pronouncements about the program were quite specific in this regard, stating that WIN was designed to "move an estimated 1.1 million adults on welfare rolls to economic self-sufficiency through job training and employment."* Quite naturally, then, the rate of such movement has been a major index of WIN performance. In addition, however, operating statistics on placement rates, completed job entries, and the characteristics of acquired jobs are also widely reported. In this section we provide a brief review of these more popular indices of program performance, leaving to Chapter III the task of relating such indices to more exacting measures of program impact.

Table 2.5 provides a useful summary of WIN operational performance as conveyed by aggregate program flows. The number of new registrants declined from 1973 to 1975 as the backlog of unregistered AFDC recipients was eliminated. The number of annual appraisals has held fairly constant, however, at approximately half a million. It is interesting to note how the number of annual participants has fluctuated, while the subtotal of new participants has consistently risen. This is largely explained by the trend we noted earlier (Section IIB) of concentrating available resources on new AFDC applicants, thereby attempting to reduce the welfare inflow, rather than later striving to speed up the welfare outflow.

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* E.g., 1971 Manpower Report of the President, p. 52.

The most frequently cited index of program performance, both by program administrators and outside evaluators, is the placement rate, typically calculated as total placements divided by total participants.* As Table 2.5 attests, this rate has held fairly constant under WIN-II at about 35 percent.** The composition of WIN employment has changed considerably, however, as noted in Table 2.6. Specifically, the percentage of OJT and PSE jobs in total employment has risen from 21 percent in FY 1973 to 30% in FY 1975. This trend is consistent, of course, with the mandate of the Talmadge Amendments that one-third of WIN expenditures be reserved for OJT or PSE components.

Returning to Table 2.5, we may also observe that the rate of completed job entries has improved significantly since the first year of WIN-II. This rate reflects the percentage of placed individuals who remained on their jobs at least ninety days, and thus provides at least a short-term measure of job retention.

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* For a number of reasons this is neither the best way to calculate a placement rate nor to measure program impact, as will be discussed further in Chapter III. Our purpose here is to convey impressions of WIN performance, as typically (and officially) reported.

** Performance data on WIN-I is difficult to interpret; see Charles R. Perry et al The Impact of Government Manpower Programs (Philadelphia: University of Pennsylvania Press, 1975), Chapter 14 for a fuller discussion.

Table 2.5 WIN Performance Indices, FY 1973-75

	FY 1973	FY 1974	FY 1975
Number registered	1,235,000	856,500	839,000
Number appraised	510,000	547,500	550,000
Participants (i.e., persons served):			
Total	353,900	534,900	483,550
New	238,500	353,000	360,400
Job openings filled	136,800	177,300	170,641
Percent of total participants	39	33	35
Continuously employed for 90 days	65,200	118,500	113,316
Percent of job filled	48	67	64
Left welfare	34,300	52,000	52,700
Percent of continuously employed for 90 days	53	44	47

Source: Employment and Training Report of the President, 1976

Table 2.6 Nature of WIN Employment FY 1973-75

	FY 1973	FY 1974	FY 1975
Direct job entry	108,800 (80%)	119,834 (68%)	113,485 (67%)
WIN/JOP (OJT)	24,200 (18%)	42,154 (24%)	37,185 (22%)
PSE	3,800 (39%)	12,625 (7%)	14,404 (8%)
Unknown	———	2,658 (1%)	5,567 (3%)
Total Number Employed	136,800	177,271	170,641

Source: 1974 and 1975 Manpower Reports of the President

Finally, we observe that relatively few individuals made a complete transition from welfare to workfare during the three years of WIN-II. Only 139,000 participants left welfare during this period, far short of avowed program objectives. In part this was due to the relatively low wages associated with WIN placements ($2.94 for men, $2.42 for women), which, together with job instability, effectively precludes financial independence. A second factor is simply the modest rate of job placements itself. Third, the AFDC program is not designed to move recipients off of welfare immediately after they secure employment. On the contrary, AFDC grants are reduced only partially as earnings increase, in an effort to encourage at least partial self-support. As a consequence, welfare grant reductions are much more common than welfare removals. Finally, WIN's increasing focus on AFDC applicants implies that many successful participants may have been deterred from welfare and thus presumably not included in the count of those who left welfare -- implying greater welfare savings than the "left welfare" statistics suggest.*

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* As we shall note later, a substantial number of WIN participants had little recent welfare experience. However, there is no real evidence of a positive "deterrent effect", as we shall observe in Chapter IX.

III. THE EVALUATION PROCESS

The size and programmatic dimensions of WIN clearly make it an important object of public concern. Among the concerns that immediately come to mind are: Was the effort and cost (over $1 billion since 1969) well spent? Could we have done better? In seeking to address such concerns it is important both to formulate the right questions and to resolve them in a meaningful way. To this end, we attempt in this chapter to identify the major issues surrounding the impact of WIN, then discuss an appropriate methodology for addressing them. A major implication of this discussion is that program operating statistics, like those discussed in the previous chapter, are not adequate indices of program impact. The remainder of the chapter describes our efforts to assemble a data base capable of evaluating WIN's impact, as well as the limitations of the data base actually acquired.

A. Major Policy and Research Questions

Insofar as the impact of effectiveness of WIN is concerned, all of the major policy questions are variants of the central concern as to whether or not WIN was "worth the effort." For completeness and later reference, however, it is useful to detail the more specific dimensions of this concern; among these we note the following questions:

- To what extent has WIN contributed to the economic self-sufficiency of participants?
- To what extent has WIN contributed to a reduction in total welfare costs?
- Which components, or mixes of components, have been most successful in achieving self-sufficiency or welfare-cost reductions?
- Have participants received any other gains, during or after program participation, than those of self-sufficiency or reduced welfare dependence?
- How do the program's benefits compare to its costs?
- Which components appear to yield the greatest relative cost effectiveness?

These questions have served as the focal points for our present inquiry, and most were stipulated in the government's original Request for Proposal. The following section indicates an appropriate approach for addressing these questions.

B. The Methodology of Evaluation

Answers to all of the foregoing policy research questions are dependent to one degree or another on the "success" of individuals who participate in the WIN program. That is to say, all measures of program impact relate somehow to what happens to the individual participant after his or her program participation. If enough individual participants are "successful" in one way or another, then so is the program.

As important as the experiences of participants are to the evaluation of program performance, they are not the only potential indices of program success. This may be particularly true of the WIN-II program, which was motivated in part by the expectation that a mandatory registration process for AFDC recipients would discourage welfare applications. From this perspective, program success would be gauged by changes in the rate of welfare applications, in addition to the experiences of program participants. The present study was not designed to measure the significance of this "deterrent" effect, but instead focuses exclusively on the experiences of program participants.*

Our approach, then, to evaluating performance of WIN-II consists of measuring the post-WIN success of program participants. From this perspective, the critical methodological issue is to identify appropriate dimensions of participant "success" and formulate a mechanism for measuring their achievement.

B.1 Gross vs. Net Changes:

At first glance, it might appear that we had already satisfied these measurement problems in Chapter II, where the placement and welfare reduction rates associated with WIN were described. However, these indices of gross flows are not necessarily reflective of WIN impact. The observation that a WIN participant later acquired a job is reassuring, but hardly a definitive index of WIN success. What we must recognize is that welfare recipients can and do get jobs on their own, and have done so even when WIN did not exist.

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* What limited evidence is available suggests that this omission is not serious, i.e., that the deterrent effect may not be significant (see Section IX E).

Similarly, individuals have been moving off the welfare rools at very high rates, even before the advent of WIN.[*] In the context of so much self-generated placement and welfare-outflow activity, it is hardly noteworthy that a WIN participant was placed or left welfare. The interesting question — the critical evaluation issue -- is what a participant would have done in the absence of WIN. Only if it can be shown that the placement would not have taken place — or that it would have taken place more slowly — in the absence of WIN, can WIN be credited with the observed placement. That is to say, "the key to the assessment of program impact is the measurement of change in individual status which is uniquely attributable to participation in the program."[**]

The distinction between gross and net changes in participants' status is easily illustrated with the data on annual earnings assembled for this evaluation. As will be noted in detail below, the earnings of male WIN participants we interviewed increased from $ 2,010 (annual rate) in the period just prior to WIN entry to an average of $ 3,420 immediately after leaving WIN. This suggests that the program was highly successful, having increased participant earnings by $1,410, or nearly seventy percent. But this gross change in earnings, however impressive, cannot really be attributed to WIN. In seeking to evaluate the impact of WIN on program participants it is essential to distinguish observed, _gross_ changes in participants' status from that fraction of such changes which can be attributed to WIN participation (the _net_ change). The critical evaluation question is not whether participant earnings have increased, but whether the earnings of participants have increased _more than they would have_ in the absence of WIN services.

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[*] For some evidence on turnover in the welfare rolls, see Bradley Schiller, "Turnover Rates in Public Assistance Programs," Welfare in Review, August, 1970, or Ketron, Inc., "Estimates of Annual Natural Turnover Rates from 1969 and 1971 AFDC National Surveys" (November, 1972).

[**] Charles Perry et al The Impact of Government Manpower Programs (Philadelphia: University of Pennsylvania Press, 1975), p. 29.

The nature of this central evaluation issue becomes clear when we consider the employment status of those individuals entering WIN. For the most part, WIN entrants are unemployed and seeking job assistance. Thus, their earnings at the time of WIN entry are abnormally low, possibly even zero.*

Suppose now that everyone who enters WIN eventually does find work, and thus enjoys positive earnings after participating in WIN. Should the entire amount of such post-WIN earnings be counted as a benefit of WIN participation? Surely not. Unless one believes that those individuals _never_ would have found employment without WIN services, then the observed change in earnings (from zero to some positive amount) overstates the impact of WIN on participant status. What we really want to know is how long it would have taken the participants to find a job on their own and the level of earnings they would have attained under such circumstances. Then we could gauge the impact of

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* The average earnings figure cited above for pre-WIN earnings ($ 2,010) refers to the six-month period prior to WIN entry, and may thus include a shorter spell of zero earnings immediately prior to entry. In fact, only 8 percent of the participant sample was working at the time of entry into WIN (see Table 3.4).

WIN in terms of the *differences* in job search duration and earnings between (A) what participants actually experienced, and (B) what they would have experienced in the absence of WIN.

The evaluation problem, of course, is that no one knows what would have happened to participants in a situation that did not exist. That is to say, we have no way of actually observing situation (B), and are thus compelled to *estimate* (B). Clearly, the set of estimates chosen will have a determining influence on the impact evaluation, and this factor necessitates considerable caution in interpreting evaluation results.

B.2 The Comparison Group Approach:

The most common method for estimating what participant status would have been in the absence of program participation (i.e. state B) is to observe the actual experiences of a similar group of individuals. Indeed, a recent review of manpower literature concluded that:

> The isolation of program effects from all other influences on the individual requires the selection of a control or comparison group. The control group should match the program participants as closely as possible in socio-economic characteristics. Most evaluators of manpower programs agree that without a control group it is almost impossible to show that improvements experienced by program participants....result from participation in the program and not from other causes, e.g., the passage of time and changes in economic conditions."*

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* Perry, *op. cit*, p. 30.

Ideally, such a comparison group would be identical to the participant group in all ways, except program participation. In practice, of course, such a condition is never fulfilled, and thus the comparison group approach may not provide a completely accurate estimate of B.

The comparison group actually utilized for this evaluation was selected from the pool of individuals who were registered for WIN services, but had not become participants. The sampling selections made from the registrant pool were done in such a way as to identify individuals who bore important similarities to the participant sample. (See Section IIID below). In particular, individuals were selected from the registrant pool on the basis of how well their age, sex, race and educational characteristics matched those of the participant sample. Thus, we ended up with a matched comparison group of non-participating WIN registrants, whose employment and welfare experiences could serve as estimates of B.

The comparison group sample of 5,033 individuals was further analyzed to determine the completeness of its "match" with the participant sample. In general, it was found that the two groups had experienced very similar employment and welfare histories in the periods at least six months prior to the WIN entry of the participant group. However, it was also found that the comparison and participation groups underwent very different employment and welfare experiences in the six-month period immediately prior to WIN entry, thus raising troublesome evaluation problems.

The actual experiences of the two study groups are summarized in Table 3.1 with each six month period separately described (e.g. t-1 refers to the six-

Table 3.1

Participation/Comparison Group Differences on Three Measures of WIN Program Impact Before and After WIN Participation

Average Annual Earnings ($000)

	Males		Females	
Period	Participant Group	Comparison Group	Participant Group	Comparison Group
t+1	$3,420	$3,230	$1,700	$1,430
t-1	$2,010	$2,670	$ 666	$1,020
t-2	$3,120	$3,200	$ 868	$1,080
t-3	$3,250	$3,320	$ 926	$1,050
t-4	$3,350	$3,530	$ 893	$1,010

Average Number of Weeks Worked Per Year

t+1	22.8	21.4	18.2	15.8
t-1	15.5	19.6	9.3	13.2
t-2	23.1	22.8	11.6	13.8
t-3	24.1	23.3	12.0	13.6
t-4	24.6	24.8	11.5	13.0

Average Number of Weeks on Welfare Per Year

t+1	23.4	24.2	36.3	37.3
t-1	27.8	29.0	39.8	40.1
t-2	21.9	23.7	34.2	37.1
t-3	19.7	21.9	30.7	34.6
t-4	18.2	20.6	28.9	32.8

month period immediately prior to WIN entry (of participants), t-2 to the six-month period prior to that, etc). Notice, in particular, how similar the experiences of the participant and comparison groups are in periods t-2, t-3, and t-4. In t-1, however, the participant group experiences a markedly greater deterioration of status; e.g., male participant earnings drop by $ 1,110 (in annual terms), while male comparison-group earnings drop by only $530. What this implies is that the "match" between the comparison group and the participant group is not perfect, and in particular, that the participant group may have in fact been selected for WIN participation on the basis of their temporary loss of relative status. Such a "selection bias" would tend to inflate measures of WIN impact, at least to the extent that changes in status were based on observations immediately prior to (t-1) and following (t+1) WIN participation. The same kind of selection bias is apparent for female participants; indeed, we must go as far back as t-3 to locate a period of comparable earnings and welfare status for the female participants and comparison group members.*

B.3 The Choice of Time Periods:

The different experiences of the participant and comparison group during the six-month period immediately prior to WIN entry raise the final evaluation issue, namely the choice of time periods for measuring status change. The issue concerns both the base period (i.e., the pre-WIN period) and the time frame after WIN participation.

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* Regression evidence on this selection bias problem is contained in Appendix B.

(i) the base period:

With respect to the base period, we have already noted that use of the period t-1 will impart an upward bias to estimates of net WIN impact, as the actual earnings of participants during that period were unusually low, both in relation to their previous experience and that of the comparison group. On the other hand, using earlier periods (e.g., t-2, t-3, etc.) as a base may impart a downward bias to WIN impact, as data for those periods do not convey the relative status loss to be experienced by participants in the t-1 period. That is to say, data from periods t-2, t-3, and t-4 overestimate the comparative pre-WIN status of participants and thus will underestimate the difference between pre- and post-WIN status. In effect, the use of (t-2) as a base period implies (a) that it is no more difficult to recover from a bad earnings slump than a mild one, and (b) that there is nothing distinctive about the WIN participants that would have made their relative status loss in (t-1) predictable.

As should be apparent at this juncture, none of the available base periods is bias-free, and estimates of WIN impact will be very sensitive to the period chosen. Our own solution to this problem is to estimate WIN impact with alternative base periods, thus providing a range of estimates, rather than only one statistic.*

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* An alternative procedure would be to estimate participant status in t-1, based on observed relationships for the comparison group. Such estimates would provide a bias-free base for gauging the relative success of the participant group over the periods (t-1) to (t+1).

(ii) the post-WIN period:

Analagous problems arise with respect to the post-WIN period, but are less susceptible to statistical resolution. What we ultimately want to know is whether or not WIN services lead to a permanent improvement in the socioeconomic status of participants. All we have been able to observe, however, are status changes acheived in a relatively short period following WIN participation. From these short periods—most commonly 9 months -- we have estimated annual status changes and tacitly assumed that such changes are maintained into the indefinite future. To determine whether or not those assumptions are valid—and thus whether or not estimated net impact "decays" over time, or actually increases—a longer post-program observation period would be required.

The brevity of the post-WIN observation period also gives rise to another bias problem. We are measuring the status of WIN participants in the period just after their WIN services terminate. For most participants, manpower services terminate at the time of job entry. Thus, our observations are sure to include substantial employment and earnings for participants. The same cannot be said for the comparison group. On the contrary, our measures of the "post-WIN" status of the comparison group are timed to coincide with the period in which participants are actually leaving the program, and are thus essentially random in nature. Consequently, we end up contrasting the experiences of participants to those of the comparison group at a time when participants can be expected to manifest a short-run advantage. Hence, our measures of net impact may be seriously inflated. The only sure way to overcome this bias is to extend the post-WIN observation period, thus providing the opportunity to observe job retention and earnings stability over a longer, less biased period.*

C. The Field Effort

The methodology outlined in the previous section requires a very comprehensive data base. In order to assure that such a data base would be available, the study team collected observations on both local program operations and the employment and welfare experiences of individuals in the WIN participant and registrant populations. In particular, Pacific Consultants conducted nearly 20,000 personal interviews at 78 different local WIN sites, while CAMIL Associates interviewed and observed WIN project staff at those same sites. In addition, CAMIL collected official welfare and WIN program data on the interview samples. Ketron, Inc., the third member of the evaluation team, was responsible for designing both the individual and site samples, and for later processing all of the assembled data.

A key feature of the evaluation effort was its longitudinal nature. WIN participants and registrants used in this evaluation were interviewed three times, beginning in the Spring of 1974 (Wave I) and concluding in the Fall of 1975 (Wave III). Thus, the evaluation effort is undertaken in a longitudinal context, wherein changes in participants' status can be observed.

The longitudinal character of the analysis applies to our local site evaluations as well. All of the 78 sites included in our study were first visited in the Spring of 1974, in order to assess the nature of program operations at the time of WIN entry (for participants). A second round of on-site evaluations was undertaken in the Spring/Summer of 1975 in order to determine whether and how local program operations had changed while sample participants were still active in the program. In this way, we acquired the capability of relating participant outcomes to changing program features.*

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* This discussion also implies that the net change in wage rate might be a superior measure of net WIN impact than either annualized earnings or weeks of employment, at least for restricted post-WIN observation periods.

D. The Study Samples

As intensive as our field efforts were, we still end up with only a sample of WIN participants, registrants, and sites, and this necessarily raises the question of how representative our observations are of the larger WIN populations. The representativeness issue has two dimensions in a longitudinal survey effort, namely, (1) how representative are the original samples, and (2) how well is that representativeness maintained throughout the several waves of interviews. Since it is obviously critical to know whose performance (and potential) is being conveyed in our survey statistics, we will address these questions briefly before describing our evaluation findings.

D.1 The Initial Participant and Registrant Samples:

Our initial field effort focused on identifying two samples of individuals, namely (i) persons who had recently entered WIN and (ii) persons who had recently entered the registrant pool. The objective of this two-pronged effort was to develop a sample of individuals who were representative of all WIN participants, then to identify persons in the registrant pool who possessed similar characteristics. Thus, the adequacy of our initial samples can be gauged by comparing the characteristics of the participant sample to those of the entire WIN participant population, and then by comparing the characteristics of the sampled registrants to those of the sampled participants.

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* The salient feature of WIN's evolution during our observation period were described in Chapter II.

Table 3.2 provides a ready comparison of the original study sample of 3,786 participants to the larger WIN participant population. As noted earlier, the participant sample was drawn from local WIN office files in the Spring of 1974. The demographic characteristics considered in the sampling process were sex, age, race, and education, and the outcomes of this process are described here. As Table 3.2 indicates, in almost every case the study sample closely matches the demographic features of the national population. However, the sample was drawn in the Spring and Summer of 1974 whereas the national figures are based on the 1974 fiscal year. So although they were the closest obtainable, the time periods for the two groups do not precisely coincide. This discrepancy in time may explain one variable (age) for which significant differences were found.*

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* Because the study samples were drawn during the Spring and Summer, one might expect the number of young people leaving school and entering WIN to be higher than at any other time of the year, and this would be reflected in the study sample, particularly since the sample was drawn exclusively from among those very recently registered or certified. The national figures, on the other hand, are based on a twelve month interval and the summer influx of young people would be normalized over the year. Indeed, it is likely that the shorter time period for the study samples would intensify the effects of any seasonal differences or policy changes.

TABLE 3.2

COMPARISON OF THE WIN PARTICIPANT SAMPLE WITH NATIONAL WIN PARTICIPANT POPULATION

		Initial Participant Sample	WIN Participants	Percentage Difference
SEX:	Male	30.0%	27.9%	+2.1
	Female	70.0%	72.1%	-2.1
AGE:	Under 20	18.7%	7.7%	+11.0
	20-29	45.5%	42.5%	+3.0
	30-39	24.4%	30.4%	-6.0
	40 and over	11.4%	19.4%	-8.0
RACE:	White	51.1%	55.6%	-4.5
	Minority	48.9%	44.4%	+4.5
EDUCATION:				
	Less than 12th grade	55.2%	59.4%	-4.2
	12th grade or more	44.8%	40.6%	+4.2

Because individuals were selected from the registrant pool on the basis of their "match" with sampled participant characteristics, it may be expected that the original participant and the registrant samples would be nearly identical. For the most part, this expectation is fulfilled, although there are a couple of exceptions. Notice, in Table 3.3 for example, that the participant sample has been on welfare for a shorter period of time than the sample of 5,033 recent registrants, and also is more likely to have left a full-time job within the last six months.* In these important dimensions, the participant sample appears to have experienced a more recent transition from 'workfare' to 'welfare' and thus might be expected to demonstrate a quicker recovery.

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* These differences arise for two reasons: First, only a few demographic characteristics were explicitly considered in the matching process. Second, the degree of match, even on these few characteristics was constrained by the number of recent registrants available for sampling purposes at local sites.

Table 3.3

COMPARISON OF THE INITIAL PARTICIPANT AND "MATCHED" REGISTRANT SAMPLES

		Initial Registrant Sample	Initial Participant Sample	Percentage Difference
SEX:	Male	78.8%	30.0%	+1.2
	Female	71.2%	70.0%	-1.2
AGE:	Under 20	15.8%	18.7%	+2.9
	20-29	44.5%	45.5%	+1.0
	30-39	25.7%	24.4%	-1.3
	40 and over	14.0%	11.4%	-2.6
RACE:	White	55.1%	51.1%	-4.0
	Minority	44.9%	48.9%	+4.0
EDUCATION:				
	Less than 12th grade	57.8%	55.2%	-2.6
	12th grade or more	42.2%	44.8%	+2.6
FAMILY SIZE:				
	2 or less	27.3%	27.6%	+0.3
	3-4	45.1%	47.0%	+1.9
	5 or more	27.6%	25.4%	-2.2
REGISTRATION STATUS:				
	Mandatory	70.8%	65;1%	-5.7
	Voluntary	29.2%	34.9%	+5.7
MONTHS ON WELFARE:				
	1-6	40.0%	47.5%	+7.5
	7-12	12.0%	9.9%	-2.1
	13-24	15.1%	13.4%	-1.7
	25 or more	32.9%	29.2%	-3.7
MONTHS SINCE LAST FULL-TIME JOB:				
	Never worked	18.2%	16.8%	-1.4
	Now working	14.6%	7.7%	-6.9
	1-6	26.5%	31.9%	+5.4
	7-12	10.3%	12.8%	+2.5
	13-24	10.0%	11.1%	+1.1
	25 or more	20.4%	19.6%	-0.8

If true, this implies that the participant sample is somewhat biased in the direction of exhibiting successful WIN impact.

It is worth emphasizing again that the initial registrant sample was not designed to reflect the potential of the entire WIN registrant pool, but rather to identify persons in the registrant pool who were similar to WIN participants, though not actually in the program. Further comparisons of the matched registrant sample with a cross section of all registrants reveal quite clearly that our registrant and participant samples are not representative of the registrant pool. In general, our samples are likelier to reflect those demographic characteristics conducive to employability and frequently used in the WIN screening process. Thus, our samples are more likely to be male, young, and high-school educated, than is true of the larger WIN registrant pool.*

D.2 The Wave III Samples:

While it is clear that the participant sample drawn for this study reflects adequately the basic demographic charcteristics of the larger WIN participant population, and that the matched registrant sample is not too different from it, one final question must be addressed. Namely, how well were the characteristics of the study sample maintained throughout the three waves of personal interviews and the attendant loss of sample subjects?

Table 3.4 summarizes the actual diminuition of the study sample through the three waves of interviews. Of the 3,786 individual participant files pulled from local WIN offices, only 3,084 individuals or 81.5 percent, were located and interviewed in the first wave of personal interviews. However, exceptionally high rates of re-interview -- 87.0 and 92.3 percent -- were achieved in the second and third waves of the interview process, thus maintaining most of the Wave I participant sample (See Table 3.4).

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* Recall the discussion (in Section IIC) of the differences between the general participant and registrant groups. Fron the general pool of registrants we have pulled those who were most like the participants.

Table 3.4

SAMPLE ATTRITION, WAVES I, II AND III

Participants:	Number	Per cent of Foregoing Total
Initial Sample	3,786	—
Wave I Interviews	3,084	81.5
Wave II Interviews	2,684	87.0
Wave III Interviews	2,478	92.3
Comparison Group:		
Initial Sample	5,033	—
Wave I Interviews	3,471	70.0
Wave II Interviews	3,035	87.4
Wave III Interviews	2,820	92.9

The original effort to locate and interview the comparison group (nonparticipating registrants) was not as successful as was the case with participants. However, once located and interviewed in Wave I, extremely high rates of response were maintained through the second and third waves, thus again preserving most of the Wave I sample.

Despite the unusually high response rates achieved in the longitudinal interviewing process, there is still the very real possiblity that the final evaluation base--those individuals interviewed in all three waves--differ from the original sample and/or their respective national populations, and thus that our evaluation results are not truly representative. As an initial response to this question, Table 3.4 contrasts the characteristics of the Wave III sub-sample to the initial participant sample.

Family size, registration status, months on welfare, and months since last full-time job have been added to the list of characteristics considered. Overall, the process of attrition does not seem to have affected the demographic makeup of the participant sample. The greatest percentage difference was encountered on the sex variable. Because our later analysis is conducted for males and females separately, this difference is immaterial to our evaluation.

Table 3.5 offers a similar comparison for the initial and Wave III registrant samples. Clearly, the process of sample attrition left us with significantly higher percentages of females and Blacks but otherwise maintained the basic characteristics of the matched registrant group.

Table 3.5

COMPARISON OF THE INITIAL AND WAVE III PARTICIPANT SAMPLES

		Initial Participant Sample	Participants Remaining at Wave III	Percentage Difference
SEX:	**Male**	30.0%	26.2%	− 3.8
	Female	70.0%	73.8%	+ 3.8
AGE:	**Under 20**	18.7%	17.8%	− 0.9
	20-29	45.5%	43.5%	− 2.0
	30-39	24.4%	25.9%	+ 1.5
	40 and over	11.4%	12.8%	+ 1.4
RACE:	**White**	51.1%	49.8%	− 1.3
	Minority	48.9%	50.2%	+ 1.3
EDUCATION:				
	Less than 12th grade	55.2%	54.6%	− 0.6
	12th grade or more	44.8%	45.4%	+ 0.6
FAMILY SIZE:				
	2 or less	27.6%	28.0%	+ 0.4
	3-4	47.0%	44.4%	− 2.6
	5 or more	25.4%	27.6%	+ 2.2
REGISTRATION STATUS:				
	Mandatory	65.1%	64.0%	− 1.1
	Voluntary	34.9%	36.0%	+ 1.1
MONTHS ON WELFARE*:				
	1-6	47.5%	43.7%	− 3.8
	7-12	9.9%	10.1%	+ 0.2
	13-24	13.4%	13.9%	+ 0.5
	25 or more	29.2%	32.3%	+ 3.1
MONTHS SINCE LAST FULL-TIME JOB:*				
	Never worked	16.8%	17.2%	+ 0.4
	Now working	7.7%	8.3%	+ 0.6
	1-6	31.9%	30.5%	− 1.4
	7-12	12.8%	12.1%	− 0.7
	13-24	11.1%	10.3%	− 0.8
	25 or more	19.6%	21.6%	+ 2.0

***Calculated at WIN registration date.**

TABLE 3.6

COMPARISON OF THE INITIAL AND WAVE III REGISTRANT SAMPLES

		Initial Registrant Sample	Wave III Registrant Sample	Percentage Difference
Sex:	Male	28.8%	23.4%	- 5.4
	Female	71.2%	76.6%	+ 5.4
Age:	Under 20	15.8%	14.8%	- 1.0
	20-29	44.5%	40.3%	- 4.2
	30-39	25.7%	28.3%	+ 2.6
	40 and over	14.0%	16.5%	+ 2.5
Race:	White	55.1%	50.2%	- 4.9
	Minority	44.9%	49.8%	+ 4.9
Education:	Less than 12th grade	57.8%	57.7%	- 0.1
	12th grade or more	42.2%	42.3%	+ 0.1
Family Size:	2 or less	27.3%	25.4%	- 1.9
	3-4	45.1%	43.7%	- 1.4
	5 or more	27.6%	30.9%	+ 3.3
Months on Welfare:	1-6	40.0%	37.5%	- 2.5
	7-12	12.0%	10.1%	- 1.9
	13-24	15.1%	14.1%	- 1.0
	25 or more	32.9%	38.3%	+ 5.4
Months since last Full-Time Job:	Never worked	18.2%	19.1%	+ 0.9
	Now working	14.6%	16.3%	+ 1.7
	1-6	26.5%	22.5%	- 4.0
	7-12	10.3%	9.4%	- 0.9
	13-24	10.0%	9.9%	- 0.1
	25 or more	20.4%	22.8%	+ 2.4

E. Data Limitations

As mentioned earlier, our longitudinal sampling design was intended to permit observations on both pre-WIN and post-WIN status of individual participants. For this purpose we conducted three waves of interviews over a period of 18-20 months. We discovered in the course of our field effort, however, that many participants were not moving through the program as rapidly as anticipated. In particular, most of those participants assigned to OJT or PSE slots were compelled to enter a holding position until a sufficient number of such slots became available. Consequently, they did not begin their OJT or PSE activity in the Spring of 1974, but instead at some later point. This, of course, postponed their date of WIN termination, a postponement rendered more serious by the fact that OJT and PSE assignments are of long duration (six months and twelve months, respectively). Accordingly, we have few observations on the true post-WIN status for individuals who entered OJT or PSE components. Instead, our "post-WIN" observations on such individuals tend to take place while they are still in subsidized employment, and we do not really know what happens when the subsidy ends. This constraint is particularly troublesome, as these components were given explicit emphasis under the Talmadge Amendments which created WIN-II. In later sections of the report we shall note how this constraint has affected specific evaluation results.

IV. AVERAGE NET IMPACT OF WIN

Our analysis of WIN's impact on participants involves several steps, each one providing increasing detail on the nature of participants' experiences. We begin in this chapter by asking how well participants as a group have fared, relative to the comparison group of nonparticipating registrants. Then we expand on this analysis in subsequent chapters by asking how WIN's impact varies with the leveland mix of services actually provided to aprticipants. We also consider the influence of the 1973-75 recession on WIN's performance. At each step of the analysis we separately consider WIN's impact on male and female participants in view of the fact the two groups have very different pre-WIN employment histories, generally receive a different mix of services, and confront distinctly different sets of job opportunities in the labor market. Racial differences within each sex group are noted as well.

In reviewing the several different estimates of WIN impact provided in this and subsequent chapters, it is essential to keep in mind the purposes of each: Each estimate is designed to provide a particular perspective on WIN and no one single estimate is necessarily the most appropriate. We attempt to interpret the significance of each estimate as the analysis proceeds, but do not provide a summary and overview until Chapter IX.

A. Alternative Measures of Benefits

As noted in the previous chapter's discussion of methodology, we gauge WIN's net impact by contrasting the changed status of participants to that of a comparable group of nonparticipating registrants (the comparison group). But we have not really addressed the question of which status dimensions are relevant to such a comparison. Annual earnings are the most commonly used for this purpose, and are widely regarded as the single best measure of benefits. Perry and his associates, for example, have observed that "annual earnings provide the most comprehensive measure of labor market performance and clearly constitute the variable of greatest immediate concern to the individual, the family, and society as a whole."*

While we share the high regard for annual earnings as the best single dimension of labor market status, we also recognize that other dimensions are important, particularly for the welfare population served by WIN. It must be remembered, for example, that the incomes of welfare recipients do not necessarily rise with their earnings. On the contrary, welfare regulations require that welfare benefits be reduced as earnings increase, thus reducing the value of earnings to the individual. In the case of male AFDC recipients, welfare eligibility actually ceases when they work 100 hours per month, thus raising the possibility that income will not increase at all as earnings rise.** Thus, to the extent that we want to measure the net gains to individual participants, we must look at net income changes as well as net earnings changes.

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* Charles Perry et al, op-cit, p. 28

** Welfare regulations preclude a loss of income at the point of ineligibility.

Taxpayers, of course, have a different perspective on the earnings/income trade-off. Generally speaking, taxpayers would consider themselves well served if WIN participant earnings substituted for welfare payments, even if the total incomes of participants were unchanged. That is to say, there is great interest in welfare savings (net changes in welfare grants), independently of what is happening to earnings or income. Also, there is considerable interest in the number of people leaving welfare, and thus in the change in the number of weeks on welfare both before and after WIN. This measure of net change, in conjunction with net changes in weeks of employment, provides an important perspective on the extent of movement from welfare to workfare induced by the WIN program.

All of the above status dimensions are employed in our measurements of WIN's impact. That is, we evaluate WIN-II in terms of how much it contributed to:

- increased annual earnings
- increased weeks of work
- reduced weeks on welfare
- reduce welfare grants
- increased total income

We recognize that even this list is not exhaustive, however, but instead focuses on measurable, economic dimensions of status. In addition, WIN may provide other benefits to participants, including socio-psychological satisfactions derived from a feeling that someone is actually trying to help welfare recipients improve their status. In this regard it is important to note that 74 percent of the interviewed participants reported being "satisfied" with WIN, and many noted a high regard for supportive services, particularly medical assistance. Those who expressed dissatisfaction with the program spoke mostly in terms of their failure to get jobs, i.e., dissatisfactions captured by our economic measures.

B. Average Net Impact

Our first effort to evaluate WIN impact focuses on the average net status gain achieved by WIN participants. As discussed in the previous chapter, we develop these estimates by comparing the pre- and post- WIN experiences of WIN participants to those of the comparison group. To the extent that the participants achieve a larger gain (or less of a loss) in status than does the comparison group between the two periods — after controlling for other influences— WIN's impact will be deemed to be positive.

The details of the regression models used to generate these estimates are described in Appendix A. We should note here, however, that the various estimates are produced from several different sets of regressions. In particular, we call attention to the fact that information on pre- and post- WIN welfare grants was not available for our entire sample, and thus that estimates of net changes in welfare grant or income are based on only 65 percent of our total sample.

B.1 Site Controlled Estimates:

Our initial estimates of WIN impact are based on comparisons of participant and comparison group experiences at specific sites. That is, we effectively evaluate each site separately, then average across sites to determine WIN's average net impact. The entire analysis is carried out in a **multivariate** framework, with explicit controls for the demographic characteristics of the two study samples. In this particular analysis, no distinctions are made for the quality of service received by individual participants, the mix of such services, or for national trends in unemployment. We are simply asking how the average WIN participant fares, relative to comparable individuals from the WIN registrant pool, in his local labor market.

Table 4.1 provides a set of answers to this basic evaluation question. Three things should be noted about these estimates. First, we are evaluating WIN's impact here in terms of only four separate dimensions of status, namely: (1) change in annual earnings, (2) change in weeks worked per year, (3) change in weeks on welfare, and (4) change in the welfare grant. Second, we have estimated WIN's impact on males and females separately. And third, as explained in the previous chapter, we have estimated net "WIN impact" over two different time periods, namely (a) t-2 to t+1 (twelve months prior to WIN to six months after) and (b) t-1 to t+1.* As noted in those discussions, we expect the first set of estimates (a) to provide a lower bound to actual WIN impact, while the second set of estimates yields an upper bound. Together then, the two sets of statistics provide an estimated range for net WIN impact.**

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* For females, the analogous time frames are (t-3) to (t+1) and (t-1) to (t+1); the difference arises from dissimilarities in pre-WIN employment experiences (see Section IIID).

** Note that the range is defined by two point estimates, and is not equivale to statistically defined confidence intervals.

TABLE 4.1:

MULTIPLE, SITE-CONTROLLED

REGRESSION ESTIMATES OF AVERAGE NET IMPACT

For Male Participants	from (t-2) to (t+1)	from (t-1) to (t+1)
Net Change in Earnings	$330*	$445**
Net Change in Weeks Worked	2.06*	2.86**
Net Change in Weeks on Welfare	-.38	-.41
Net Change in Welfare Grant	—	-$164

For Female Participants	from (t-3) to (t+1)	from (t-1) to (t+1)
Net Change in Earnings	$327***	$471***
Net Change in Weeks Worked	3.07***	4.19***
Net Change in Weeks on Welfare	-.09	-.50
Net Change in Welfare Grant	—	-$106***

* significant at .10 level
** significant at .05 level
*** significant at .01 level

Note: t-statistics are in parentheses, while the complete set of regression coefficients and additional measures of statistical significance are provided in Appendix Tables

Sample sizes are as follows:

Males: For earnings, weeks worked and welfare weeks, N=933 (495 WIN participants, 438 comparison group). For welfare grant, N=561 (276 WIN participants, 285 comparison group).

Females: For earnings, weeks worked, and welfare weeks, N=2976 (1648 WIN participants, 1328 comparison group). For welfare grant, N=2165 (1146 WIN participants, 1019 comparison group).

All tests of significance are one-tailed.

Focusing first on the estimates for male participants, it appears that they enjoy a change in earnings which is from $330 to $445 greater than that experienced by the comparison group, after controlling for demographic characteristics. This is accompanied by a net increase in employment -- again measured against the experiences of the comparison group -- of from two to three weeks per year. On the other hand, there is no comparable reduction in welfare dependency, as neither the time on welfare nor the average grant is reduced significantly, on a net basis. That is to say, males on welfare but not in WIN appear to leave the welfare rolls just as frequently as male recipients served by WIN.* In large part, this discrepancy can probably be explained by the fact that male AFDC recipients lose their welfare eligibility after working 100 hours per month; thus, earnings gains above those yielded by the first 100 hours of employment are not reflected in welfare savings.

The estimates of net impact for female participants are larger, and not subject to the same policy constraint. According to Table 4.1 the WIN program increased the earnings of female participants by $327 - $471, increased the weeks worked by female participants by 3.1 - 4.2, decreased the number of weeks female participants spent on welfare by .1 - .4, and reduced their annual welfare grant by $106. In assessing these results, it is important to note that AFDC regulations provide that welfare grants will not be reduced on a dollar-for-dollar basis with earnings gains, at least not for female recipients.* Hence, the net reduction in welfare is bound to be smaller than the net increase in earnings.

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* The gross changes in all the status variables, for both the participant and comparison groups, are detailed in Table 3.6, only the net impact estimates are of interest here.

C. The Problem of "Unassigned Participants"

While the foregoing estimates of net average impact suggest a modest degree of success for the WIN program, they may not fairly represent the benefits of actual WIN services. The problem is that "participation" does not necessarily imply receipt of services. On the contrary, many of the individuals officially regarded as participants have received no WIN services, and thus could not expect to benefit directly from their participation. At many sites, such unserved individuals are referred to as "unassigned participant", a program classification that grew significantly during our observation period (by the Spring of 1975, 25 sites had instituted an "unassigned participant" pool). Although there is no such status in the WIN handbook or regulations, the use of this component apparently increased because of confusion about the intent of the WIN redesign.**

Although the purpose of the unassigned participant category was to increase the number of "participants" potentially available for placement and other services, the use of the category varied across the sites. The most common use was to classify persons appraised but awaiting services or not enrolled in a component as "unassigned". In some sites, however, the unassigned participant category was used to test the motivation of registrants and to have them engaged in placement-generating activities independent of the WIN program itself. For example, unassigned participants in some areas were expected to engage in an informal job search as a prerequisite to participation. Only if the person did show willingness to find work, but could not, was he or she enrolled in the program. In some sites, the unassigned participants were even expected to attend orientation or world-of-work classes without having been certified, or without WIN incentives.

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* Welfare grants for female recipients and males working less than 100 hours per month are reduced by two-thirds of gross earnings less work expenses and $30 per month.

** At some sites this was also used as a way to obtain credit for persons finding their own jobs who were not formally WIN participants. Since sites were not supposed to record as a placement jobs found by registrants prior to participation, by reassigning large numbers of registrants to an "unassigned participant" category, the potential for "authorized" placements greatly increased.

At some sites, the unassigned participant category was used as an alternative to stop status. Since there was no time limit for this category, unlike stop status, a "participant" could be held indefinitely. In some cases, records showed that persons had been in this category for more than three months without any contact with WIN.

The use of the unassigned participant category was not, however, wholly negative. Some sites which used the unassigned participant category as a bridge between appraisal and certification were able to monitor and maintain contact with the registrant during the certification period. In some cases, productive job search activities were carried out during what would have normally been a period of total inactivity.

In general, however, the presence of "unassigned participants" in the participant sample will tend to reduce estimates of WIN services impact, as most such individuals received no services from WIN. Accordingly, we have attempted to improve our estimates of average net impact by distinguishing "unassigned participants" in our sample from those participants who are likely to have received some (any) WIN service. In all, there were 24 males and 135 females classified as unassigned participants.

Table 4.2 provides estimates of net average impact for those participants who were not consigned to the unassigned participant pool and thus are likely to have recieved some (any) WIN service. In general, these estimates are s slightly lower for males and significantly greater for females than those of Table 4.1. This apparent anomally may be explained in part by the fact that served male participants are more likely than served female participants to receive just minimal placement assistance (see Table 6.3), thus constraining the distinction between 'served' and 'unserved' ("unassigned") participants. In addition, it should be noted that only 24 males were excluded on the basis of "unassigned" status.

We have also calculated the net change in socioeconomic status experienced by "unassigned participants." To make this calculation, we have first contrasted the status of the unassigned participants at the time their 'served' counterparts were leaving WIN with their status prior to WIN, then compared this (gross) status change with the experiences of the comparison group. In general, we would not expect the unassigned participants to exhibit

Table 4.2

MULTIPLE REGRESSION ESTIMATES OF AVERAGE NET IMPACT FOR PARTICIPANTS RECEIVING SOME SERVICE

For Male Participants	From (t-2) to (t+1)	From (t-1) to (t+1)
Net Change in Earnings	$292*	$418**
Net Change in Weeks Worked	2.18**	2.99***
Net Change in Weeks on Welfare	-.85	-.86

For Female Participants	From (t-3) to (t+1)	From (t-1) to (t+1)
Net Change in Earnings	$412***	$553***
Net Change in Weeks Worked	3.81***	4.90***
Net Change in Weeks on Welfare	-1.02 *	-1.42**

* significant at .10 level
** significant at .05 level
*** significant at .01 level

a net gain in status, as they have not received any special manpower assistance.* What is surprising is that they actually experience a substantial decline in relative status. As Table 4.3 reveals, the average female unassigned participant experienced a net loss of $588-$626 in earnings, nearly five weeks of employment and an increase of four weeks in welfare dependency. Net losses for male unassigned participants appear larger still, though not always statistically significant.

It is difficult to explain why the unassigned participants should have experienced such substantial net losses, as there is no obvious reason why 'unserved' participants should do any worse in the labor market than any other group of WIN registrants with similar characteristics (i.e., the comparison group). Two suggestions might be made, however. First, the unassigned participants might differ from 'served' participants in systematic, but unmeasured ways. In particular, we noted that lesser-motivated or less attractive participants might be delegated to unassigned status as a way of focusing program services on the most readily employable. This kind of assignment procedure would render the unassigned participant pool one for the least likely to succeed.

The second suggested explanation lies in the phenomenon of 'retroactive certifications.' As noted earlier, it is possible for local program managers

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* Note that two-tailed tests of significance are used here in recognition of the fact no impact is hypothesized for unassigned participants; this contrasts with our hypothesis of positive impact for served participants (and thus our earlier use of one-tailed tests).

Table 4.3

Multiple Regression Estimates of Net Change in Status for 'Unassigned Participants'

For Male Participants	From (t-2) to (t+1)	(t-1 to (t+1)
Net Change in Earnings	- $ 1,297*	- $1,158
Net Change in Weeks Worked	- 5.97	- 5.17
Net Change in Weeks on Welfare	8.51**	8.45**

For Female Participants	From (t-3) to (t+1)	From (t-1) to (t+1)
Net Change in Earnings	- $626***	- $588***
Net Change in Weeks Worked	- 4.80***	- 4.95***
Net Change in Weeks on Welfare	4.07**	4.01**

* Significant at .01 level
** Significant at .05 level
*** Significant at .10 level

Note: two-tailed tests of significance are employed here (see text).

to boost performance ratings by exploiting the successes of unassigned participants. That is, should an unassigned participant find a job, he is immediately moved from unassigned to assigned status, credited with a WIN service (e.g., placement assistance), and thus regarded as a successful program participant. By so behaving, WIN managers not only inflate program impact (for 'served' participnats), but further increase the likelihood that the (remaining) unassigned participant pool will manifest relative status losses, of the kind we have observed. This implies, of course, that our estimates of average net WIN impact for 'served' participants are subject to yet another upward bias.

C.1 Racial Differences:

While the distinction between served and unserved participants is clearly important, it does not apply equally to whites and blacks. More specifically, there is considerable evidence that the average served black participant does not benefit as much from WIN as does the average served white participant. Moreover, there is some evidence that among black males 'served' participants do not even do better than 'unserved' participants. In view of the fact that we are measuring net changes in status, and thus limiting our comparisons to individuals of similar backgrounds (including race), this disparity is particularly troublesome.

Table 4.4 provides a convenient summary of the differences in average net impact for blacks and whites.* Reading down the first column, we see that the net earnings gain (lower-bound) for black males is statistically insignificant. This contrasts sharply with the $580 net annual earnings gain achieved by white males. Clearly, our earlier (Table 4.2) observation that served

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* Our measures of black status include other minorities; however, blacks comprise over 90 percent of the minority group.

Table 4.4 Net Changes in Earnings by Race and Participation Status

	Net Change in Earnings from (t-2) to (t+1)	
	'Assigned' Participants	'Unassigned Participants
Black Males	-$270	-$1,090
White Males	$580**	-$1,415
Black Females	$255***	$ 575**
White Females	$634***	-$ 639**

* Significant at .10 level
** Significant at .05 level
*** Significant at .01 level

N = 672 white males, 284 black males, 1307 white females, 1704 black females male participants achieved an average net earnings gain from WIN of $292 was a generalization of two very different racial experiences.

A similar racial disparity is evident among female participants, although somewhat less pronounced. Served ("assigned") black females achieve a net earnings gain on the order of $255 per year, less than half the gain ($634) achieved by their white counterparts.

To some extent these racial differences in average net impact are attributible to differences in the intensity of services provided. However, as we shall discuss further in Chapter 6, racial differences persist even among blacks and whites who have received the same level of WIN service.

D. Net Changes in Income

We have yet to consider the extent of net income changes experienced by WIN participants, haveing focused instead on earnings, weeks of work, and weeks on welfare. As noted earlier, we have had to analyze changes in income separately, as income, particularly welfare data was not available for our entire sample.

Our measure of total income includes earnings plus the AFDC grant and any allowed work expenses; it does not include receipts from other sources such as unemployment, insurance, alimony, child support, etc., all of which tend to be marginal and highly unstable. The data on welfare grants used for this analysis was taken from local welfare office records and combined with earnings histories provided by participant interviews. This process constrains the findings in two ways. First, because useful welfare grant data was limited, our analysis of income changes is restricted to comparisons of the (t+1) and (t-1) periods, and thus represent upper-bound estimates of WIN impact. Second, because welfare office records were available for only two-thirds of the interview sample, the earnings gains we calculate here are not identical to those described earlier. However, these constraints are not too binding, as our immediate interest is in the relationship of earnings gains to income gains, not in the absolute size of either.

For convenience, we have summarized both the gross and the net income changes in Table 4.5. Thus, we see that the average female WIN participant had an income of $2,774 just prior to entry into WIN, with approximately one-fourth ($778) of that amount coming from earnings. In the six month periods after WIN (t+1) her average income was $3,390, with nearly fifty percent of that amount coming from earnings. Our regression estimates indicate that much of the gross earnings gain is attributible to WIN, and that most of the earnings gain is reflected in income. That is to say, not only did WIN generate a net earnings gain for served female participants, but in addition women were able to keep most of their earnings gains. This, of course, suggests that the average "welfare tax" on female earnings is quite low, on the order of 20 percent."* This is quite low, though not inconsistent with the regulatory provision that female recipients be permitted to retain $30 per month of their earnings in excess of work expenses, after which point the welfare grant is reduced by 67 cents for each additional dollar earned.

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* The average implicit tax on earnings can be calculated as gross change in earnings less the gross change in income, divided by the gross earnings change, i.e., ($747-$616) ÷ $747.

Table 4.5 Gross and Net Income and Earnings Changes, for Served Participants

	Females	Males
Average Income in (t-1)	$ 2,774	$ 4,441
Average Income in (t+1)	$ 3,390	$ 5,103
Average Gross Change in Income	$ 616	$ 662
Average Net Change in Income Attributible to WIN	$ 561***	$ 290
Average Earnings in (t-1)	$ 778	$ 1,868
Average Earnings in (t+1)	$ 1,525	$ 3,046
Average Gross Change in Earnings	$ 747	$ 1,178
Average Net Change in Earnings Attributible to WIN	$ 614***	$ 465**

* Significant at .10 level
** Significant at .05 level
*** Significant at .01 level

Male WIN participants had a much higher income base ($4,441) in the immediate pre-WIN period, and achieved a larger (gross) gain in earnings. However, their experience was very different from that of females in two other important respects. First, the net earnings gain ($465) attributible to WIN was relatively small in comparison to the gross earnings gain ($1,178). This, of course, was due to the fact that males in the comparison group achieved fairly sizable earnings gains on their own. Second, the change in income experienced by males was substantially less than their earnings gains, implying a substantial welfare tax rate (44 percent). The higher welfare tax on male earnings is explained both by statutory termination of welfare after 100 hours per month of employment and the higher earnings base they began with. Finally, we should note that the average post-WIN incomes of male WIN participants, while substantial ($5,103) still left them below the official poverty line for a male-headed family of five ($5,912).

E. WIN Benefit Decay

Before leaving the subject of average net impact, we need to consider the extent to which observed benefits disappear, or "decay", over time. It is widely recognized that the net status change attained immediately after program termination may not be representative of longer-term impact: A quick job placement may turn out to be a job of short duration, or, conversely, a low-paying job may turn out to be the first step on a meaningful career ladder. Clearly, both the individual participant and the larger society have an interest in determining what happens to WIN's observed net impact over a longer period of time, as the rate of benefit decay may well determine whether WIN is providing a "permanent" solution to some families' welfare problems or merely a brief respite from welfare dependency.

E.1 The Time Constraint:

As important as the subject of benefit decay is, it is unfortunate that evaluation efforts are generally confined to very short time frames. Our own evaluation effort is no exception to this rule. Eventhough our longitudinal time frame was longer than that usually permitted, it is not long enough to get a meaningful perspective on longer-term impact. Out of the 2,478 participant interviews completed at Wave III, only 402 provided information for a full twelve months after program participation. As a result, our estimates of WIN impact largely reflect short-run status changes, and we have no sure basis for assessing how those changes will be maintained over time. This problem is particularly serious for the 309 participants who entered OJT or PSE slots, as virtually all of our observations are restricted to the period during which their employment was publicly subsidized. Thus, we not only have very little information of longer-term benefits for most of our sample, but in addition, we have no clues as to what happens to OJT/PSE participants after their employment subsidies terminate.

E.2 Preliminary Explorations:

While recognizing that nothing conclusive about longer-term WIN impact can be learned unless and until a fourth wave of interviews is completed, we have experimented somewhat with the data presently available. In particular, we have separately analyzed available data for 436 males (233 participants, 203 comparison group) and 1525 females (804 participants, 721 comparison group) who have a full 9 months of data after WIN participation. This subset of the full sample was used to obtain very preliminary estimates of benefit decay, without consideration of the level or nature of services received by individual participants. That is to say, we are focusing on the decay rate for average net benefits and ignoring potential variation around either the average benefit or the average decay rate.

The nine month follow-up period was divided into three quarters. Each of these quarters constitutes a distinct observation period. A regression analysis was performed to estimate the change in the dependent variable from the pre-WIN periods to each of the post-WIN quarters. These estimates were performed for three of the economic variables of interest in the study. Specifically, the following regressions were run for males and females, separately:

- Change in average annual earnings from period t-2* to First Quarter
- Change in average annual earnings from period t-1 to First Quarter
- Change in average annual earnings from period t-2* to Second Quarter
- Change in average annual earnings from period t-1 to Second Quarter
- Change in average annual earnings from period t-2* to Third Quarter
- Change in average annual earnings from period t-1 to Third Quarter

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* This period was t-3 for females.

Table 4.6
Decay in Program Effects: Regression Estimates of WIN Program Effect by Calendar Quarter After Participation, Males (N = 439*)

Change from period:	Quarter After Participation					
	First Quarter		Second Quarter		Third Quarter	
	Coeff.	t-value	Coeff.	t-value	Coeff.	t-value
(t-2) to (t+1)						
Change in earnings ($000)	.273	.790	.648	1.73	.221	.600
Change in weeks worked	2.70	1.39	4.38	2.20	2.29	1.10
Change in weeks on welfare	-.190	-.090	-2.09	-.990	.180	.080
(t-1) to (t+1)						
Change in earnings ($000)	.383	1.10	.786	2.09	.300	.800
Change in weeks worked	3.98	2.04	5.63	2.81	3.53	1.68
Change in weeks on welfare	-.440	-.210	-2.41	-1.16	.020	.010

Table 4.7
Decay in Program Effects: Regression Estimates of WIN Program Effect by Calendar Quarter After Participation, Females (N = 1528**)

Change from period:	Quarter After Participation					
	First Quarter		Second Quarter		Third Quarter	
	Coeff.	t-value	Coeff.	t-value	Coeff.	t-value
(t-3) to (t+1)						
Change in earnings ($000)	.400	3.42	.318	2.76	.365	3.03
Change in weeks worked	2.68	2.55	3.38	3.17	4.04	3.75
Change in weeks on welfare	.250	.260	-.680	-.650	-1.05	-.980
(t-1) to (t+1)						
Change in earnings ($000)	.402	3.77	.449	3.94	.515	4.39
Change in weeks worked	3.74	3.66	4.29	4.09	4.82	4.51
Change in weeks on welfare	-.030	-.040	-.930	-.920	-1.32	-1.25

* Only includes males having at least nine months of follow-up data: WIN participants = 233, comparison group of unserved registrants = 203.

** Only includes females having at least nine months of follow-up data: WIN participants = 804, comparison group of unserved registrants = 721.

Identical sets of regressions were run to estimate change in number of weeks employed and change in the number of weeks on welfare. Tables 4.6 and 4.7 contain the results of these regressions.

It can be seen from these tables that for males program effects apparently increase, then decrease, but these effects are so imprecisely estimated that little can be concluded from them. For females, on the other hand, the net employment (weeks worked) gain seems to grow with each quarter, while the net earnings gain either increases or decreases, depending on which base period is chosen. Such findings hardly lead to a firm conclusion. However, they would seem to suggest that the decay rate for female participants is low, possibly even zero, i.e., that observed net benefits are maintained over time. However, the extremely short observation period available here precludes any meaningful conclusions about long-run decay rates.[*]

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[*] Reliable estimates of the decay rate could be derived from further observations on the study population. As we shall note in Chapter VIII, such estimates are critical to assessments of WIN's cost effectiveness.

V. LABOR MARKET INFLUENCES

In assessing our estimates of WIN impact it is important to recognize that WIN's performance during our observation period (1973-75) may have been influenced by the adverse labor market conditions then prevailing. It is widely recognized that the success of all manpower programs depends to some extent on the state of local and national labor markets.* If one views WIN as a mechanism for increasing the flow of unemployed workers into jobs, the general dimensions of such influence are apparent: in adverse labor markets WIN's success will be constrained by an increasing number of jobless people and a diminished number of available slots. Because of the severity of the 1973-75 recession, we need to ask whether such influences significantly reduced WIN's performance capability during our observation period, and thus whether our estimates of average net impact understate WIN's potential in improved labor markets. We may also inquire as to whether and how local WIN projects adapted to the deterioration of labor market conditions.

A. Labor Market Conditions, 1973-75

The salient feature of the national labor market during the period July 1973-September 1975 was the recession. Indeed, the depth and duration of the recession, whether measured in terms of lost jobs or lost output, were the worst since the Great Depression.

The impact of the recession on the national labor market is most conveniently conveyed by a single statistic, the unemployment rate. This rate rose from a pre-recession low of 4.6 percent in October 1973 to a peak of 8.9 percent in May 1975. Importantly, the period of most rapid increase in unemployment occurred between spring 1974 and spring 1975, right in the middle of our WIN performance observations.

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*See Bradley Schiller, The Economics of Poverty and Discrimination (Englewood Cliffs, N.J.: Prentice-Hall, 2nd edition 1976) for a more general discussion of labor market influences on poverty and welfare flows.

Not all workers are affected equally, of course, by the national unemployment rate or variations therein. On the contrary, persistent differentials in the burden of unemployment have been documented for various demographic and skill groups. In general, such statistics demonstrate that lower-skilled workers, blacks, women, and younger workers bear the hardest burdens of a recession. For example, in May 1975, when tha national unemployment rate stood at 8.9 percent, unemployment among unskilled male laborers was 16 percent, among Black males 14 percent, and among teenagers 20.3 percent.

These differential unemployment rates are of immediate relevance to WIN, as WIN tends to serve the least skilled groups. As a consequence, statistics on national unemployment rates and trends may understate the constraints on WIN performance. While it is always true that turnover and firm-specific expansion will create some job openings, even in the worst of times, it is important to recognize just how severely limited job opportunities become for the disadvantaged in times of recession. Such recognition leads to the expectation that WIN performance evaluations undertaken during a recession will underestimate WIN potential, at least to the extent that they cannot control completely for all labor market effects.*

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*The controls used in this evaluation are site-specific unemployment rates and changes therein. These are by no means perfect--particularly in view of the data sources available--but do provide partial statistical controls for the impact of the recession.

A. 1 Site Variation in Recessionary Impact:

Although the national recession of 1973-75 was prolonged and severe, it did not impact upon all local labor markets with equal force. On the contrary, many local areas were spared the brunt of the recession, or at least were affected much later in the cyclical downturn. This variation in recessionary impact was evident among our 78 sample WIN sites, and provides an opportunity to test the significance of adverse local labor market conditions.

A summary statement of the variation in recessionary impact across local WIN sites is provided in Table 5.1. Note that a small minority of sites (7) was relatively unaffected by the recession, at least as gauged by changes in the local unemployment rate (one site even experienced a reduction in the local unemployment rate!) while most experienced an increase in unemployment on the order of 50-90 percent. At seventeen sites, the local unemployment rate more than doubled between the Spring of 1974 and the Spring of 1975.

B. Estimated Effects of Rising Unemployment

We have utilized the observed variation in recessionary impact to derive preliminary estimates of the influence of rising unemployment rates on the average net impact of WIN. To do this, we have categorized site experiences according to whether the observed increase in local unemployment was "low", "moderate", or "high", as illustrated in Table 5.1. We then use this distinction to determine whether or not a "low" rate of increase in local unemployment

— thus, a relatively recession-free environment — is positively associated with client outcomes. We do this by stratifying our samples into three groups, then estimating a separate regression for each. The average net impact estimates yielded by this procedure are depicted in Table 5.2.

The estimated net impact of WIN on male earnings appears to be influenced appreciably by labor market conditions, as the average impact rises as we move from "high" unemployment-increase sites to "low" ones. Moreover, the impact estimates for male participants in "high" and "moderate" sites fail to achieve a ninety percent confidence level, and thus do not carry much statistical significance. By contrast, the estimate for "low" sites does attain our confidence level cut-off, thus increasing our confidence in the observation that WIN impact was higher in sites least affected by the 1973-75 recession.

Our estimates of WIN's net impact on the earnings of female participants in different labor markets generally confirm that WIN's performance was adversely affected by the recession. Note in particular how WIN's net impact on female earnings rises from an average of $389 in areas severely hit by the 1973-75 recession to $559 in areas relatively unaffected by the cyclical downturn.*

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* The low net impact for females in "Moderate" areas is curious. In assessing the impact of the recession, however, it is most appropriate to compare WIN's impact in "Low-change" areas - i.e., ones which were essentially spared from the recession — to all others. From this perspective, the "Moderate" and "High" areas would be combined and contrasted with the "Low" areas, an approach that would raise our estimate of recessionary impact on females and lower our estimate for males.

Table 5.1

SITE VARIATIONS IN UNEMPLOYMENT TRENDS, 1974-75

Percentage Increase in Local Unemployment Rate	Number of Sites	
Less than 18%	7	"low" change
18 - 36%	14	
37 - 50%	9	"moderate" change
51 - 62%	15	
63 - 99%	16	"high" change
100 - 149%	12	
150 - 199%	4	
Over 200%	1	

Source: Appendix G

Note: These figures are based on data obtained directly from state and local sources, and thus may differ from usual BLS figures. We chose this approach so as to get a better measure of site-specific labor market conditions than that provided by BLS estimates of SMSA unemployment. Our data reflects officially approved rates for county unemployment, as developed for the purpose of CETA and EEA funding. In those instances where such estimates were not available, we used BLS data for the larger SMSA as indicated with asterisks in the table. It is important to note that our estimates for any site are derived from the same source for each year, thus rendering our estimates of site-specific change internally consistent. Unemployment trends for individual sites are described in Appendix G.

Table 5.2 Variations in Average Net Impact, by Unemployment Trends

	High	Moderate	Low
'Served' Male Participants	- $ 132 (0.3)	$ 432	$ 624
'Served' Female Participants	$ 389***	$ 288*	$ 559***

*	significant at .10
**	significant at .05
***	significant at .01

Note: estimates based on comparisons of (t-2) for males and (t-3) for females; unassigned participants are excluded and neither WIN service levels nor additional site characteristics are included in the estimating equations.

The estimates of labor market influences provided in Table 5.2 refer to the average net impact of WIN, as previously explained (Section IIIB). While these estimates are appropriate for gauging the influence of the recession on WIN performance, it is also important to note how the individuals in our samples fared. In this regard it is interesting to note that the gross change in earnings experienced by male participants and registrants was -$652 in "high" sites, +$382 in "moderate" sites, and +$472 in "low" sites. That is to say, the gross earnings of both the participant and comparison group males were severely depressed by the recession; our net impact estimates merely suggest that WIN helped soften the blow to participant earnings. The same relationships are evident in the gross earnings of females, they have risen by $432 in "high" sites, $668 in "moderate" sites, and by $725 in areas least affected by the recession.

C. Local Responses to the Recession

While it appears that WIN's performance was significantly constrained by the 1973-75 recession, it is quite possible that our estimates do not fully express the force of the recession on WIN activity. In particular, it is possible that local WIN project managers, sensing the threat of adverse labor market conditions, may have increased or reallocated their project resources. To the extent that they behaved in this fashion, they might have ameliorated the impact of the recession, and thus obscured the full force of deteriorating labor market conditions. Accordingly, we sought to complete our analysis of labor market influences by determining whether and how local WIN projects adapted the level or structure of their services in response to changing labor market conditions. Information on this question was obtained in the field by Camil staff, and is based on conversations with local WIN personnel. Each site was visited twice, and a comparison of the information acquired at each visit is the framework in which program changes are identified. For example, Camil staff endeavored to determine during their first visit (Spring 1974) the extent to which OJT was being used. An identical question was asked during the second visit (Spring 1975), thus providing a basis for determining whether the level of OJT had changed during the year.

The observation that particular program components have changed in some way does not, of course, answer the question of whether or not such changes were initiated as a local response to changing labor market conditions. Resolution of this issue is complicated by the fact that local program changes can be initiated at several administrative levels and for many different reasons, regardless of their coincidence in time with labor market changes. Such complications were apparent during our period of interest. From 1974 to 1975, decreasing WIN budgets, combined with a continuing emphasis on placements, resulted in a reduction in the use of institutional training, orientation, work experience, services, and even on-the-job training and public service employment--despite the mandate that one-third of the budget be expended for these categories.

State administrative changes in response to the budget restrictions tended to dominate most local site plans, masking, in many cases, local initiatives instituted because of the labor market. For example, even though many of the sites had experienced formal changes in staffing levels, or reduction in the availability of employability development and service components, in no case did the local managers attribute these formal changes to an analysis of the labor market, or to locally initiated actions. Invariably, the changes were attributed to administrative policy decisions or budget cuts at higher administrative levels.* In many instances, in fact, the instituted changes were viewed as opposite to the direction the local managers would have taken given a free hand; particularly the restrictions on training, on-the-job training, and public service employment during a period when it was becoming increasingly difficult to find job openings.

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*A review of administrative memoranda which initiated these changes revealed that reasons for these changes were related to national program policy emphasis and not to the adverse labor market conditions.

In the same regard, it is of interest to note that there appears to have been a tendency for WIN projects to have benefited from an expansion of CETA budgets. In 23 of the 78 sites in our total WIN II evaluation sample, CETA became the primary source of institutional training slots, either supplanting or extending other sources. In addition, 16 of the sites reported that CETA had become an increasingly important source for OJT training slots. And finally, 12 of the sites reported that CETA was an important source of PSE slots. Local program administrators, of course, had no control over CETA budgets, so that the observed changes in training and placement activity cannot be identified as a local WIN response to labor market conditions. The observed change was nevertheless timely and important.

As concerns our analysis of the adverse labor market, then, we are confronted by a number of significant project changes from 1974 to 1975, few of which can be related directly to the change in the labor market situation. Moreover, because local budgets and staffing levels were modified independently of local analyses of labor market conditions, changes which could be instituted at the local level were of a secondary nature. In other words, local projects were unable to alter significantly their budgets or staffing specifically in response to the changing labor market conditions. At most, they were able to shift their approved staff from one function to another or to concentrate on selected *available* components. They were not able to institute, however, training for future jobs in response to a existing poor market, increase on-the-job training, hire additional placement staff, or institute any number of other "indicated" activities. These decisions were preempted by their plans, budgets, and letters of instruction from higher administrative channels.

What changes were instituted at the local level in response to adverse labor markets were very limited, then, and confined almost exclusively to changes in component emphasis and, in some projects, to the way in which a component was conducted. In this respect, it is important to observe that two-thirds of all 78 sites reported greater difficulty in obtaining placements, both because fewer jobs were available and because employers had raised prerequisites for those jobs that remained open. In addition, two-thirds of the sites reported that the number of available OJT slots had declined, while half of the sites reported a decrease in the number of available PSE slots. Finally, half of the sites reported that the quality of available jobs had diminished.

Although these adverse developments suggested a need for radical restructuring of WIN components*, we have already noted that such action was beyond the discretionary power of local project managers. Accordingly, local responses tended to focus on increased placement effort, as evidenced by three phenomena:

(1) increased efforts on the part of regular placement staff;

(2) increasing involvement of non-placement staff in placement activities; and

(3) new techniques used in placement activity.

Of the three, the first was by far the most frequent, with 69 percent of all sites reporting that placement staff were working harder. At the 16 sites where local unemployment rates had more than doubled during the observation period, 78 percent reported increased placement staff effort. By contrast only 45 percent of all sites reported increased involvement in the writing of OJT contracts.

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*For a discussion of appropriate structural responses, particularly the need for an expansion and upgrading of institutional training in such circumstances, see Pacific Consultants' report on WIN I (The Impact of Urban WIN Projects, June 1972.)

With respect to new placement techniques, forty-two sites said they had had to increase job development and personal contacts with employers by use of their Employer Relations Representative (ERR's) or activities similar to those used by ERR's. For example, a few sites sent mailings to all major employers in the area, with no results. Half of the sites complained that the level of placements was declining because of labor market conditions even with the additional effort. A few sites, however, said that it was somewhat easier to place participants either because of increased program pressure to accept employment or because the participants were more willing to accept any job.

The following table summarizes these and other changes in local program activity. For the purpose of this table, we have distinguished (1) those sites with an unusually high rate of change in the local unemployment rate (sites where the unemployment rate at least doubled between the spring of 1974 and the spring of 1975) and (2) those which experienced relatively modest increases (less than a forty percent increase) in unemployment from the averages of all 78 sites. Thus, for example, we see that 69 percent of all sites report that their placement staffs were working harder in spring, 1975 than in spring, 1974. But we also note that the increased effort was even more prevalent (78 percent) at sites which experienced the worst deterioration in local labor market conditions. However, we also note (row two) that the higher reported placement effort at such sites was not as frequently accompanied by the involvement of additional staff in the placement function.

We should note that local WIN staff had very mixed feelings about the increased placement emphasis that so frequently emerged as labor conditions worsened. In many cases the staff thought it was absurd to spend more time and effort trying to locate jobs that were quickly disappearing. On the other hand, many felt that the augmented placement effort could offset higher unemployment rates, particularly in view of the fact that WIN processed so few people in relation to the total population of job-seekers.

Table 5.3 Changes in Local Activity, by Change in Unemployment

Activity Change	Sites with High Increase In Unemployment*	Site with Low Increase In Unemployment**	All Sites (N=78)
Placement:			
Placement staff working harder	78%	65%	69.7%
More staff doing placement	28	48	45
Recession constrained placement rate	78	43	77
Lower quality placements observed	44	61	54
Number of PSEs declined	44	57	46
Employer's raised job prerequisites	61	70	67
Training:			
Recession reduced training	17	26	19
More staff involved in OJT	17	13	10
Number of OJTs declined	78	65	67

* local unemployment rate increased by at least 100%, N=16

** local unemployment rate increased by less than 40%, N=23

Turning to the training activities summarized in the table, we find that a minority of the sites (19 percent) perceived that training activity had been affected by the recession, a suggestion which is supported by our observation that those sites with the highest rates of increase in local unemployment were less prone to report that training activities had been affected. Finally, we may observe that OJT activity declined most everywhere, with the heaviest reductions taking place in the most rapidly deteriorating labor markets.

VI. SERVICE INTENSITY AND IMPACT

Estimates of average net impact of WIN, as provided in the foregoing chapters, are essential for developing a general perspective on how well the national WIN effort is doing, and are a critical element in decisions on whether or not to continue or expand program activities. However, estimates of average net impact disguise tremendous variability in the performance of individual participants and local program sites. By analyzing such variability, we may increase the potential for improving program performance. To this end, this chapter focuses on the variation in the level of services provided to individual participants. What we seek to determine is whether or not the nature and intensity of WIN service provision markedly influences post-program success.

In considering the level of service received by WIN participants a distinction must be drawn between employment services and supportive services. The central focus of WIN is on employment services, e.g., the training, vocational counseling and placement activities provided by ES/WIN. In addition, however, WIN participants may receive supportive services such as child care, transportation, or medical examinations, as determined by the local welfare department's Separate Administrative Unit (SAU). We will focus here on employment services, postponing a discussion of supportive services to Chapter VII.

A. The Distribution of Employment Services

The principal employment services available* to WIN participants are appraisal, orientation, education, vocational training, work experience, on-the-job training (OJT), public service employment (PSE), and job placement. However, few WIN participants receive all of these services; in fact, most receive only one or two. This is largely explained by the fact that most participants do not need all the available services. But the low delivery rates on many services also reflect program and policy decisions. As noted in Chapter II, WIN-II has moved ever closer to a placement-only concept. Thus, fewer employability development resources have been available, and project personnel have become increasingly selective in admitting job ready participants.

The overall distribution of services provided to WIN participants is given in Table 6.1, which shows provision of employment services for each sex by race, education, work history, and welfare history. The information

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* "Available," in this context, means permitted to be provided. Not all services are available at all sites.

Table 6.1 The Distribution of WIN Employment Services, by Sex, Race, Education, Work and Welfare Experience

MALE

		RACE			EDUCATION				WEEKS WORKED BEFORE WIN					WEEKS ON WELFARE BEFORE WIN				
	Overall	White	Black	Other	1-8	9-11	12	13 & Over	0	1-12	13-24	25-44	45 & Over	0	1-12	13-24	25-44	45 & Over
Orientation	25.9%	22.1%	37.1%	10.0%	27.0%	33.0%	21.4%	13.8%	37.2%	32.4%	25.9%	19.1%	9.1%	25.6%	16.5%	28.3%	30.3%	30.8%
WIN vocational training	7.5	6.9	9.0	10.0	5.2	7.7	8.1	9.2	11.6	13.7	4.7	5.1	--	7.0	6.1	6.7	4.5	10.8
Other vocational training	3.2	4.1	--	--	3.5	2.9	4.0	1.5	7.0	2.0	2.4	1.7	5.5	1.2	4.3	3.3	1.5	3.8
Adult basic education (ABE)	2.3	1.7	4.1	--	3.5	3.3	1.2	--	2.3	3.9	5.9	1.1	--	3.5	2.4	1.7	3.0	2.3
High school equivalency (GED)	1.6	1.0	3.4	--	--	3.8	.6	--	1.2	2.9	--	2.2	1.8	3.5	2.4	--	1.5	.8
English second langauge (ESL)	.2	.2	--	--	--	.5	--	--	--	--	--	.6	--	--	.6	--	--	--
Other education	.4	.5	--	--	--	--	1.2	--	1.2	--	--	.6	--	--	--	1.7	--	.8
Work experience	2.5	2.5	2.8	--	2.6	1.9	4.0	--	5.8	2.0	--	2.2	3.6	3.5	3.0	--	1.5	3.1
WIN/OJT	19.0	21.1	13.8	20.0	14.8	21.1	16.8	26.2	14.0	18.6	27.1	18.0	18.2	22.1	20.1	16.1	8.2	16.9
WIN/PSE	6.0	4.0	9.7	10.0	4.3	3.3	7.5	13.8	2.3	4.9	7.1	7.9	7.3	9.3	7.9	5.0	6.1	2.3
Suspense employment	3.0	3.7	.7	--	1.7	4.3	3.5	--	1.2	2.9	4.7	4.5	1.8	2.3	2.4	13.3	--	2.3
Job search or intensive job exposure	8.7	8.7	8.3	10.0	14.7	7.2	8.1	4.6	12.8	10.8	9.4	6.2	--	4.7	3.7	15	15.1	9.2
Job entry	73.9	75.7	70.3	60.0	75.7	70.8	74.0	80.0	57.0	65.7	78.8	79.2	89.1	76.7	78.7	76.7	71.2	65.4
Completed job entry*	36.2	38.2	31.0	33.3	37.4	29.7	37.6	52.3	25.6	29.5	45.9	39.3	54.5	46.5	40.2	40.0	27.3	30.0
TOTAL		71.6 403	25.8 149	1.8 10	20.4 115	37.1 209	30.7 173	11.5 65	15.3 86	18.1 102	15.1 85	31.6 178	9.8 55	15.3 86	29.1 164	10.3 60	11.7 66	23.1 130

*Not all job entries had existed for 90 days at time of record coding.

Table 6.1 continued

FEMALE

		RACE			EDUCATION				WEEKS WORKED BEFORE WIN					WEEKS ON WELFARE BEFORE WIN				
	Overall	White	Black	Other	1-8	9-11	12	13 & Over	0	1-12	13-24	25-44	45 & Over	0	1-12	13-24	25-44	45 & Over
Orientation	42.4%	27.1%	54.8%	46.5%	46.3%	47.7%	40.6%	21.6%	50.7%	38.1%	36.8%	39.3%	26.8%	29.9%	35.2%	36.4%	40.8%	47.1%
WIN vocational training	18.9	23.8	14.5	23.3	7.9	16.0	23.9	24.3	21.2	18.2	17.5	18.3	12.2	12.6	18.7	23.6	16.9	19.3
Other vocational training	5.9	6.5	5.2	11.6	3.0	5.8	6.6	6.3	6.4	4.6	5.4	7.1	2.4	4.6	4.9	9.2	5.5	5.3
Adult basic education (ABE)	6.8	3.8	9.2	13.9	6.4	10.1	4.7	1.8	8.3	7.2	5.8	5.2	1.2	5.7	5.3	6.2	7.0	7.6
High school equivalency (GED)	4.3	2.6	5.6	4.6	4.4	8.8	.6	--	3.4	6.0	4.9	4.4	3.7	4.6	3.9	6.7	5.0	4.0
English second language (ESL)	.5	.8	--	4.6	3.0	.3	--	--	1.2	--	--	--	--	--	--	1.0	--	.7
Other education	.5	.4	.6	--	--	.6	.6	.9	.6	1.0	--	--	--	2.3	.7	--	--	.5
Work experience	10.3	9.6	10.7	18.6	5.9	9.1	13.8	4.5	10.8	10.6	11.2	10.3	7.3	12.6	11.3	9.2	10.0	10.6
WIN/OJT	11.2	11.4	11.1	9.3	14.8	9.5	11.6	11.7	10.8	12.2	11.2	11.9	8.5	13.6	9.2	9.7	13.9	11.5
WIN/PSE	4.8	4.5	5.0	9.3	1.5	4.4	6.1	6.3	4.3	2.9	7.2	6.3	7.3	2.3	7.0	4.1	3.0	4.9
Suspense employment	4.6	4.3	4.7	4.6	3.9	3.7	5.1	8.1	4.6	5.3	5.4	2.4	6.1	4.6	5.6	4.1	3.0	3.8
Job search or intensive job exposure	8.9	7.5	10.2	4.6	9.9	8.2	9.3	9.9	9.1	8.7	11.2	9.1	7.3	10.3	3.1	7.2	9.5	9.4
Job entry	60.6	62.6	60.0	46.5	64.0	56.5	63.5	61.3	52.5	62.1	64.6	73.4	72.0	64.4	65.5	57.9	59.2	59.8
Completed job entry*	28.3	32.1	25.5	23.3	27.1	22.0	32.9	41.4	22.6	27.6	35.0	40.5	32.9	31.0	33.1	32.3	28.4	25.3
TOTAL		43.6 764	52.9 928	2.5 43	11.6 203	40.3 706	727 41.4	111 6.3	38.4 674	23.8 417	12.7 223	14.4 252	4.7 82	5.0 87	16.2 284	11.1 195	11.5 201	50.2 881

*Not all job entries had existed for 90 days at time of record coding.

in the table was developed from a review of program records at each of the 78 sampled WIN sites, and corresponds with remarkable consistency to information given by the participants themselves in the sample interviews.* The nature and distribution of each employment service is discussed in the following paragraphs.

A. 1 Appraisal:

The purpose of the appraisal interview is to assess the registrant's employability potential and suitability for employment or participation in a WIN component. It is one of the first steps completed prior to WIN certification and participation.**It consists of evaluating the registrant's work history, work-related skills, and employment interests. In addition, the need for supportive services is determined with respect to job search and employment. The appraisal process is an ES/WIN and SAU staff function, with interviews being conducted jointly or sequentially (See Section IIB).

Virtually all WIN participants should receive an appraisal interview. Yet only 73.1 percent of our sample reported receiving this service, with no notable variation by sex, age, education, or race. This low figure is probably attributable to the fact that the appraisal process is typically quite short and not particularly distinctive from a client's point of view ("just another bunch of questions"). Thus, despite our efforts to elicit accurate responses on this question,* the clients' perceptions are probably subject to severe recall problems, and thus quite conservative.

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* The only exception occurs with respect to placement services: the program records indicated more participants being placed by the program than the interviews did. This is discussed below.

** Because appraisal occurs prior to WIN certification, information on the utilization of appraisal had to be obtained from participant interviews.

A. 2 Orientation:

In WIN, orientation can mean either or both of two activities: (1) an explanation to new participants of the program itself, including the participants' legal rights and obligations, or (2) an explanation of the "world of work," with emphasis on employers' requirements: punctuality, dress and grooming, work habits, and the like. At most of the 40 sampled sites that were still offering orientation in 1975 -- down from 48 in 1974 -- the latter approach was emphasized.

Orientation classes ranged in length from a single day to a full week. The longer courses typically included practice in filling out job applications, role-playing job interviews, and sharing information on the local job market. At some sites, local employers participated in the orientation sessions, explaining what they look for in hiring new workers. At several sites, WIN staff worked individually with participants during the orientation period, developing employability plans.

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* The question asked of respondent during the Wave III interview was: "Did you attend a meeting with Welfare and WIN people to see whether you should participate in the program" (This meeting is sometimes called an appraisal interview)." In the Wave I interview a sequence of three questions was used to determine whether the respondent had been appraised.

As can be seen from the Table 6.1, female participants were more likely than males (42.4% versus 25.9%) to receive orientation. This might be expected, given the generally more limited work experience of female participants. Indeed, it is evident that those with the weakest work histories -- and least education -- are likeliest to take part in orientation.

A. 3 Counseling:

Counseling is no longer considered a formal WIN component, and site records did not permit determination of which participants had been counseled.* However, counseling was offered at 57 sites in 1975 (up from 51 in 1974). Fourteen of these sites had fulltime counselors on the WIN staff; at the rest, counselors mixed other duties with counseling, with the other duties usually occupying more than half of their work time. Caseloads per counselor ranged from fewer than 60 at some sites, to nearly 300 at others.

Most counseling was vocational -- this is, related to job search -- but most of the sites offering counseling also dealt, to some extent, with participants' family - related and other personal problems.

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* Thus, the distribution of counseling services is not shown in Table 6.1.

Many WIN administrators interviewed expressed the opinion that the program's emphasis on direct placement, and the decline in availability of other components, point to the need for a strong counseling effort. In the absence of many educational or training options, the participant needs help in making the most of the job search effort.

A. 4 Vocational Training:

While 38 sites still had an institutional (classroom) training component in 1975 (down sharply from 54 in 1974), the amount of training actually available was frequently negligible. Some 27 sites had decreased their institutional training budgets between 1974 and 1975, and only 17 sites had substantial institutional training to offer (down from 36 in 1974).

While this diminishing role of training is consistent with both the emphasis on direct placement and the squeeze on the WIN budget, a number of WIN staff interviewed felt the timing of the cutback was unfortunate. At most sites, jobs were harder to find than in 1974, and unemployment rates were up; WIN staff felt that the time was right to strengthen training opportunities, to create a trained workforce to move into jobs once the economy began to improve.* With few jobs, and no significant training, WIN's options are severely limited -- particularly for the participants who are not job-ready.

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* The importance of vocational training in periods of high unemployment is also stressed in Lloyd Ulman, ed. Manpower Programs in the Policy Mix (Baltimore: John Hopkins University Press, 1973).

As Table 6.1 shows, only 7.5 percent of the male and 18.9 percent of the female participants were exposed to vocational training. What training was provided tended to be devoted to those with the best educational accomplishments (particularly among females), suggesting a kind of "screening for success." There is no similar strong correlation between provision of training and either work or welfare history, however; thus, the picture that emerges is one of permitting selected high school graduates to continue their job preparation by attending training classes.

A. 5 Education Components:

Classroom education in WIN attempts to provide employment-related education to those who have not completed their high school education, cannot speak English, or lack basic reading, writing, and computational skills.

Adult basic education (ABE) and general educational development (GED) * are used by WIN for participants judged unable to compete in the job market without further education. For participants of non-English-speaking nationalities, a third education component, English as a second language (ESL), is offered. Education components are usually provided by WIN on contract with the local board of education,** and are taught in school classrooms.

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* GED is a high school equivalency program; the successful completer receives a certificate considered equivalent to a high school diploma.

** Boards of education sometimes provide these courses without cost to WIN.

In 1975, 41 sites were offering ABE, down from 47 the preceding year. Fifty-two sites had GED, an increase over 1974's total of 46. There was also an increase in the use of ESL, from 20 sites in 1974 to 24 in 1975. To some extent, these increases can be attributed to the growing role of CETA in making educational components available to manpower trainees, including WIN participants. Another factor in the growth of GED while ABE declines is WIN's increasing concentration on the job-ready, or nearly job-ready, participant. A participant who needs basic education is probably still a long way from placement; one whose chief barrier to employment is lack of a high school diploma may be able to move into job-ready status with a relatively brief, and relatively inexpensive, exposure to GED.

Only a small proportion (approximately ten percent) of WIN-II participants actually received educational services. Blacks were much likelier to participate in educational components than were whites, females than males, and -- appropriately -- those with lower educational attainments than those with higher.

A. 6 Work Experience:

The WIN work experience component permits participants to work at the WIN office itself, or in some other public or private nonprofit agency or institution. It is usually offered to participants who have never worked, or whose work experience is scant -- for example, a person whose last job was many years ago. Participants work without pay, continuing to receive public assistant grants and WIN incentive payments, plus applicable work-related expenses.

Use of this component, like the other training options, declined in 1975. Some 32 sites offered work experience in 1975, as compared with 41 in 1974. Most of these (23) made work experience available to a participant for only a five-week period, instead of the recommended thirteen weeks. This abbreviated component was used to provide a quick brush-up on rusty work skills, to accustom participants to a worker's schedule (including arranging babysitting or other child care), and to permit a trial period during which an agency could "try out" a worker without obligation. Work experience, particularly in the WIN or welfare offices, was also used to fill in time between other components, so that the participant would maintain interest and get used to showing up every day on time.

Reflecting the fact that it was often used for clerical-type positions, work experience was provided far oftener to women than to men: better than 10 percent of the women in the participant sample participated in this component, as against only 2.5 percent of the men. For both sexes, there was a tendency to use work experience with participants who were relatively well-educated, but whose work histories were scant.

A. 7 WIN/OJT:

In this component, WIN enters into a contract with an employer to hire a participant for a specified period, with the program paying half of the worker's salary. Depending on the kind of job, contracts can be written for up to a year's length; however, in 1975 most were for much shorter periods —

10-week contracts were not uncommon. The employer agrees to train the worker on the job, so that transferable skills are acquired which can increase the participant's future employability.

While 44 sites had significant levels of OJT in 1974, only 23 maintained them in 1975. While this decline was partly a reflection of program budget cutbacks, it also mirrored the local economy at many sites -- employers were unwilling to take on new workers, even at a bargain, when they were laying off long-standing employees.

Writing an OJT contract after a placement had been found was a common practice in 1975 -- nearly a third of the sites engaged in this practice of "retroactive OJT." It was sometimes done to use up allocated OJT monies, and sometimes to persuade an employer to keep a marginal worker employed.

Some other unusual local uses of OJT were noted during 1975. One site employed a "voucher system" for OJT, permitting the participant to find his or her own job. The participant carried an OJT voucher to the job interview, as an added incentive to the employer.

In some areas, use of OJT was enhanced by CETA programs supplying OJT slots to WIN. The CETA OJT contract form is shorter (a single page, compared with three to eight pages for WIN OJT contracts), a fact appreciated by employers; further, CETA's involvement with the local governmental structure was sometimes an added attraction for employers, offsetting the presumed stigma of "welfare."

Nationally, occupational areas most frequently used for WIN OJT have included manufacturing and assembly, machine operation, automobile assembly and repair, and construction. As opportunities in those more traditional fields declined during 1975 in a sluggish economy, other occupational areas were frequently tapped: building maintenance, retail sales, clerical occupations, and paramedical positions.

In a number of areas, abrupt budget cuts threw the OJT program into chaos. A dozen states were obliged to cancel operating contracts; more than 30 others had to "suspend" contracts pending receipt of new funds. Alienation of participating employers as a result of such steps was widely reported.

The degree to which an OJT slot is really "training" varies. Some employers demand job-ready workers, which is contrary to the purpose of OJT. A number of WIN administrators made the point that OJT should be viewed as a kind of private sector PSE arrangement, rather than as a training experience.

In any event, OJT is a real job, for a real employer -- a big step removed from institutional training (which may or may not lead to a job) and from work experience (which does not give workers the satisfaction of receiving a paycheck, and which is often stigmatized as "make-work").

As noted in Table 6.1 OJT is a reasonably significant WIN component, with 19 percent of the men and 11.2 percent of the women participating. With the men, there is some tendency to favor for this component the more job-ready, as measured by educational, welfare, and work histories; however, no such pattern is evident for female participants. Also, for the men but not the women, there is a greater tendency for whites than blacks to be assigned an OJT opportunity.

A. 8 The WIN Tax Credit:

While not a service component, in the strict sense, the WIN tax credit is so closely allied with OJT opportunities that it should be noted here. Under the tax credit provisions,* an employer can receive a credit amounting to 20 percent of the wages paid to a WIN participant during the first 12 months of employment. Although these 12 months need not be consecutive, the credit is subject to recapture if the employee leaves during the second 12 months, unless the separation is voluntary or for cause.** The principal objective of the tax credit, of course, is to estimate the demand for WIN participants.

The tax credit has not met with much success, for a variety of reasons. First, the credit provisions themselves are confusing, both to potential employers and WIN staff (who must explain it). Second, WIN-II's emphasis on rapid placement has produced a high percentage of self-placements (approximately 50 percent), and a number of placements prior to the person's receiving AFDC for 90 days -- another requirement for the tax credit. Third, WIN participants are not particularly anxious to be singled out as welfare recipients in the job setting.

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* Authorized under the Revenue Act of 1971.

** There are a number of other requirements: the participant must not displace another employee, the vacancy must not be the result of a strike or dispute and must not have occurred due to temporary lay off, the wages must meet the federal minimum wage, if applicable, and if not must not be substantially less favorable than the prevailing wage, must offer the same employee benefits as similar jobs in the firm, and must meet all provisions of applicable health, safety and other standards. In addition, the credit is limited to $25,000 a year plus one-half of the tax over that amount.

Of the 78 sites visited for the study only 20 in 1974 and 15 in 1975 indicated they made extensive use of the tax credit, to the extent of informing employers of it at every opportunity and trying to develop jobs on the basis of its availability. National data show that out of over 170,000 placements in each of 1974 and 1975, only 38,000 were certified for tax credit in 1974 and only 25,000 in 1975.*

Most local WIN staff felt the inducement represented by the tax credit was too small and that the 24-month employment requirement was too long to be attractive for most employers. The only exceptions were a few employers with a long term relationship with the program who made a habit of hiring recipients, or employers who extended into on-the-job training contracts. In fact, the best use of the incentive, according to program staff, was in combination with the OJT package as an extra incentive. The OJT contract offered the employer an immediate reimbursement for some wages paid to the participant; the tax credit held out the offer of an additional return if the participant remained on the job. Moreover, for persons hired under OJT contracts, there is no problem of revealing the employee's welfare status, as it is already known.

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* Even fewer placements will actually result in use of the tax credit as not all of these placements will last long enough to qualify for the credit.

A. 9 Public Service Employment:

The WIN PSE component allows the program to purchase jobs for participants in public or private non-profit agencies, usually for a year. WIN pays the full wage -- not half, as with OJT.

Consistent with national WIN priorities, PSE was far more limited than OJT. Only 29 sites were writing any PSE contracts in 1975, down from 40 sites in 1974, and most sites that did use the component had only one or two active contracts. As with OJT, 11 sites had been forced to cancel ("de-obligate") PSE contracts in midstream.

WIN policy requires that there be two OJT contracts for every PSE contract written at a site. Thus, use of PSE declined automatically as OJT faded. A further problem is that PSE contracts, unlike those for OJT, require that the purchased position must become "permanent" (that is, unsubsidized) at the end of the contract period -- a hard guarantee for agencies to make when they have their own budget problems. The hiring agency is also required to certify that the WIN PSE job is one that "would not otherwise be performed by regular employees" * Thus, the employing agency must, in effect, create a job and then promise that it can be maintained without subsidy after the contract is over.

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* Section 56.40, WIN regulations

There is another difficulty, this one from the participant's point of view. PSE workers are not eligible to receive either the WIN incentive payment, or the "thirty-and-a-third" income disregard normally applied to welfare grant calculations for recipients who find work. This creates a serious distincentive for a recipient to accept a PSE position, and some sites made no use of the component for that reason.

At the sites still writing significant numbers of PSE contracts, the occupational areas most commonly found were clerical(the most frequently used field), paramedical, municipal construction, and municipal garage mechanic.

For both sexes, the number of participants who received PSE was about a third of the number who participated in OJT. In general, this component also favored the more employable participants, gauged by education, work experience, and welfare history.

Suspense Employment

This minor WIN component is a recording category used primarily to note participants hired by other programs, notably CETA. Several sites also used it to indicate entry of a participant into a part-time or short-term job. As indicated in Table 6.1, it was likelier to be used for women than for men (4.6 percent versus 3 percent).

Job Search

Forty of the sampled WIN sites required that participants (and, in some cases, nonparticipating registrants) undertake a formal job search — a sharp increase over 1974's total of 22 sites. Participants were required to review job listings at the ES, and in the classified sections of newspapers. In addition — particularly at sites with active job development as part of WIN — participants were required to check regularly with the program to see if suitable jobs might be available.

Fourteen of the sites requiring job search tended to put most participants in this component, virtually from the beginning of their WIN participation, if no other program experience was available. The other 26 used the component more selectively, at whatever point in program participation the participant was considered job-ready.

Notwithstanding these site requirements, only a small fraction of sampled participants had been exposed to the job search component -- under 9 percent for both men and women. This is largely explainable by the the fact that substantial proportions of participants (more than 60 percent of the men and more than 50 percent of the women) received either placement services only, or placement services in combination with initial services only (appraisal, oreintation, counseling). In such cases, a job search requirement was unlikely to be enforced, since it would be unnecessary.

There is a disquieting indication of possible misuse of this component in the statistics for male participants (Table 6.1). It can be observed that the "poorest" job prospects (in terms of education, work experience, and welfare history) — those likeliest to require assistance and preparation before being able to compete for jobs — were overly represented in the job search component.

Job Entry

Job entry is, of course, the "payoff" component in WIN, and placement is the program's chief (and, increasingly, sole) criterion of success. During 1975, more than half of the sampled sites received state directives to increase placement activity. In most cases, the increased emphasis on placement activity coincided with a deteriorating job market (see Chapter V). Caught in the conflicting pressures generated by the demands for direct placement and the existence of shaky labor markets, site-level WIN administrators responded in a number of ways: by shifting staff from other functions to job development and placement, by asking for reductions in placement goals they felt to be unrealistic (generally refused at the state level) — and by evident irritation.

In assessing the placement statistics reported in Table 6.1 it is important to distinguish between "job entry" and "completed job entry." Any participant who takes work, however briefly, may be recorded as having participated in "job entry"; only jobs that last for at least a calendar quarter result in a notation of "completed job entry." The difference, then, is between getting a job and keeping a job — a significant distinction indeed.

As mentioned earlier, the placement figures in Table 6.1 are taken from program records, and do not coincide with placement results reported by participants themselves. In general, participants report much lower rates for WIN placement assistance.*

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* Only 37.9 percent of male participants and 32.8 percent of female participants responded affirmatively to the question, "Did WIN ever place you in a job?"

The disparity, noted in other studies as well,* reflects varying perspectives on the nature of "self-placement." When participants do secure jobs, they are unlikely to attribute the placement to WIN, eventhough the program may have provided many hours of needed services, and perhaps even made the successful referral. From the participant's viewpoint, the job resulted from a successful interview with the employer, not from the program's efforts. Yet the WIN site may well consider that same event a WIN job entry and, with considerable justification, record it as such.**

Looking at the "bottom line" of the charts -- the completed job entries -- the correlation between positive outcomes and client characteristics is readily apparent. For both men and women, education, welfare history, and work history all correlate with Job Entry and CJE success. Not unexpectedly, the program succeeds most often with its ablest participants.

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* Bradley R. Schiller, WIN Job Search (Pacific Consultants, June, 1974) or Camil Associates, A Retrospective Case Review of WIN-II Completed Job Entries October, 1974

** Local WIN managers, too, are prove to exaggeration of course, and thus may overstate their role in "self-placements." Interestingly, however, the use of "retroactive placements" — the crediting of a true self-placement to WIN Job Entry -- apparently declined somewhat, as only 38 of the sample sites were engaged in this practice in 1975, down from 45 sites in 1974.

Follow-Up

After the WIN participant is placed, an employer relations representative or other staff member "follows up" the placement by contact with the employer, the worker, or both. This contact is usually by telephone, but may entail a visit to work site, particularly if any problems are reported or anticipated. The purpose is to make sure the participant is actually on the job, and to head off any problems that may be developing from either the worker's or the employer's point of view. Similar follow-up is used for participants in OJT and PSE, and sometimes for those in training courses.

The question of whom to contact for post-placement information is a touchy one in WIN. Many participants do not want the program staff talking to their employers about them, and site staff themselves are often reluctant to make an inquiry that might raise a question in the employer's mind about what may be "special" about the new employee. In many cases, particularly where the participant has found the job directly, the employer does not know that the new hire was in a "program," or even that the person was on welfare. A call from the WIN office in such a situation can make trouble for the newly-hired worker.

Recognizing this, sites doing follow-up frequently contact the worker, rather than the employer. (In the case of PSE or OJT contracts though, the employer is contacted, since the employer obviously already knows of the worker's WIN affiliation). In this situation, the employer cannot take advantage of the federal income tax credit available to employers who hire WIN participants.

At a time of dwindling resources, follow-up is considered somewhat of a frill in manpower programs, and is one of the first activities cut back. Between 1974 and 1975, 18 of the WIN sites had significantly reduced follow-up, or eliminated it completely. Most sites in 1975 were performing follow-up once or twice for three months after placement (the time when job entry is considered "completed" and the worker has no further relationship to WIN). However, some 15 sites were following up on placed participants for a full year.

B. The Identification of Service Levels

The foregoing description of who receives WIN services generally confirms our earlier observations about WIN's emphasis on placement, as relatively few participants receive basic education or vocational training, while nearly all receive some job development assistance. It is also noteworthy that the distribution of services is roughly consistent with clients' apparent needs, with the least employable receiving more intensive employability services and the most employable receiving more job development and placement assistance. It remains to be seen, however, what the effect of all these services is on the socio-economic status of participants: Are all of WIN's employment services equally effective in terms of net benefit? If not, which ones appear to have the greatest impact?

A basic problem in attempting to measure service impact is that WIN participants may receive any combination of employment services, in accordance with their needs and resource availability. Table 6.2 illustrates some of the actual service combinations received for participants who had completed their WIN activity by the time of the third interview. The most frequent pattern of services received includes appraisal, some orientation, and then job development.

Table 6.2 Major Combinations of Employment Services

	Male	Female
No employment service	6.4%	7.6%
Initial services only*	10.8%	14.9%
Initial services plus education or training	3.6%	9.2%
Initial services plus job development or placement	50.8%	35.5%
All services	13.6%	20.0%
Job development or placement only	10.0%	5.8%
Other and Unknown	4.8%	7.0%
	100.0%	100.0%

* includes appraisal, orientation, or employability planning.

The tendency for participants to receive a combination of employment services rather than only one service makes it difficult to assess the independent effect of any single service, particularly in a non-experimental study.* The problem is compounded when we recognize that the quantity of a particular service also varies widely, (e.g., one vs. many job referrals, one week vs. one month of institutional training, etc.). In view of these problems, we have arbitrarily defined five levels of WIN employment service, and seek to determine the impact of each on participants' post-WIN status. The five composite service levels are:

> Level One: Unassigned Participants: persons administratively classified as participants, but not receiving WIN services except perhaps for job search.
>
> Level Two: Essentially Placement Services Only: persons who received no more than placement services (e.g., an employability plan, referral, job development) and perhaps vocational counseling, orientation, or work experience. The range in this group was from a single discussion about potential jobs to numerous referrals, employer contacts, and counseling.
>
> Level Three: Education: persons who received some education services in addition to whatever placement and related services were also made available to them under Level Two.
>
> Level Four: Vocational Training: persons who received some vocational institution training. Such persons may or may not also have received educational courses, placement, and counseling services (Levels Two and Three).

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* In an experimental study the patterns of services would be deliberately varied to detect the relative impact of alternatives.

Level Five: Subsidized Employment: persons who received OJT, PSE, or Suspense Employment and who may also have received educational services and placement services (Levels Two, Three, and Four).*

The actual frequency of these service levels among those respondents who had completed their WIN activity is described in Table 6.3. Clearly the most common service level was Two, i.e., virtually exclusive emphasis on job development and placement activities. Over sixty percent of male participants and nearly as high a percentage of female participants reported receiving no more than placement assistance. A slight tendency for blacks to be disproportionately restricted to this service level is apparent as well. By contrast with the clear emphasis on placement, few respondents report the receipt of educational or training services. Subsidized employment on the other hand, is provided to approximately one-fifth of the male participants and to one-eighth of the female participants. Here is a very substantial racial disparity is evident, particularly among males.

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* It was theoretically possible for such persons to have received institutional training as well, but none did.

Table 6.3 Frequency of WIN Service Levels, by Sex and Race

Service Level*		White Males	Black Males	White Females	Black Females
One:	unassigned	4.8%	4.7%	8.3%	7.5%
Two:	placement only	60.6%	65.1%	53.3%	57.7%
Three:	education	3.9%	6.4%	6.1%	9.4%
Four:	training	8.7%	7.6%	19.2%	13.8%
Five:	subsidized employment	22.1%	16.3%	13.0%	11.6%
		100 %	100 %	100 %	100 %
number:		(335)	(172)	(733)	(934)

* See text for more complete definitions

Note: The distribution of service levels reported here is based on a combination of record data (where available) and interview responses (where record data was unavailable), and this is not entirely compatible with Table 6.1 which was based on record data alone. (The differences, however, are slight).

Table 6.4

Estimated Net Impact at Different Levels of Service

For Male Participants:	Level 1	Level 2	Level 3	Level 4	Level 5
Net Change in Earnings	-$1,280*/-$1,139	.../...	-$1,091/-$960	.../$772	$1,823**/$1,975**
Net Change in Weeks Worked	-6.0/-5.3	.../...	.../...	.../...	11.7**/12.8**
Net Change in Weeks on Welfare	8.43*/8.4*	.../...	9.9**/9.4*	.../...	-5.1**/-5.2**
For Female Participants:					
Net Change in Earnings	-$618**/-$586**	$231**/$361**	.../$228	$469**/$662**	$1,487**/$1,558**
Net Change in Weeks Worked	-4.9**/-5.1**	2.6**/3.5**	.../...	4.2**/5.6**	12.3**/12.7**
Net Change in Weeks on Welfare	3.9**/3.9**	.../...	2.2/...	.../...	-6.5**/-6.8**

* significant at .05 level
** significant at .01 level

Note: Only statistically significant (at the .10 level) coefficients are displayed here -- as low estimate/high estimate -- whereas the entire set of regression results are displayed in Appendix Tables
These regressions include controls for demographic characteristics and local unemployment conditions.

C. Estimated Impact of Service Levels

While the distribution of delivered services is of interest in its own right, we are more immediately concerned with the net impact of WIN's various services. Accordingly, we have used our basic model to estimate the average net benefit received by participants at the five different levels of WIN service. In particular, we have compared the post-WIN status of participants from each level of service to the status of the comparison group, with controls for demographic characteristics, work histories, and local labor market conditions. Thus, the coefficients we have generated, as detailed in Table 6.4, represent the net status gain or loss attributable to the receipt of a particular service level. As will be noted, our estimates strongly suggest that higher levels of WIN service have a pronounced impact on post program success, while the lowest level of service ("unassigned participants") actually resulted in net status losses. We will examine the results for males and females separately, then discuss some problems of interpretation.

C. 1 Males:

As was discussed in (Section IV C), those male "participants" who receive no WIN services do not benefit from their WIN "participation." On the contrary, this particular group actually experiences a substantial net loss of status, as compared to both served participants and non-participating registrants.

When we restrict our attention to only those participants who actually received services (Lvels 2-5), some notable results emerge. In general, WIN appears to have no positive impact at low levels of service, and may even have a negative net impact. Only at Level Four (training) and at Level Five (subsidized employment) does WIN appear to raise the net status of male participants. Indeed, it appears that extremely high benefits are experienced by those males who entered OJT or PSE (Level 5). Our low estimates of net impact indicate that these individuals experienced a net increase in annual earnings of $1,823, a net increase in employment of nearly twelve weeks (per year), and a net reduction of five weeks in welfare dependency. These estimates are not only five times larger than estimated average net impact, as detailed in the previous chapter, but represent a nearly two-fold improvement in the pre-WIN economic status of these individuals.

Some immediate caution must be exercised in interpreting the estimated impact of OJT/PSE. Virtually all of our "post-WIN" observations on participants who entered PSE and most of our observations on those who entered OJT are restricted to the period in which their employment is being subsidized. This occurs for two reasons. First, the OJT/PSE contracts are very long (typically six and twelve months, respectively), relative to other WIN activities. Second, the participant who enters an OJT or PSE slot is likely to regard him or herself as having completed the WIN program, even though his or her job is still being subsidized. Indeed, from the participant's perspective, OJT/PSE slots look just like regular jobs, with a regular paycheck. Unfortunately, they are not, and we really don't know what will happen to the OJT/PSE participants after their public subsidy ends. To the extent that their economic status is threatened when WIN subsidies end, we are clearly over-estimating the net impact of OJT/PSE.*

Despite the obvious limitations of our Level Five impact estimates, they do convey an obvious short-run benefit to OJT/PSE participation. First of all, subsidized employment slots obviously provide relatively high incomes. More importantly, however, our net impact methodology suggests that the OJT/PSE slots provide a level of employment and earnings that is very different from the work experience of the WIN population. This in itself is a noteworthy benefit, and may imply that some meaningful "upgrading" is taking place. The ultimate test of the "upgrading" hypothesis depends, however, on post-subsidy data which we do not yet have.

Before leaving the subject of service intensity for male participants, we should also note that the net benefits we have observed appear to be limited to whites. Additional tabulations (not shown) indicate that the lower-bound gain to white males in OJT/PSE was $2,225 (t=4.9) while for black males it is much smaller ($753) and statistically insignificant. This suggests that either black applicants are screened more carefully to assure a better job 'match' — which implies less upgrading — or that blacks are being allocated distinctly inferior OJT/PSE slots.*

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* And estimates of average net impact as well, as the average includes the gains from subsidized employment. In order to get reliable estimates of either average net impact or the impact of OJT/PSE, a longer post-program observation period is obviously required. With the limited post-program data available in this study, the most that could be done would be to identify the termination date of OJT/PSE subsidies and adjust respondent's "post-program" calendars accordingly. The effort would not be very productive, however, since few respondents had substantial post-OJT/PSE histories to report.

C. 2 Females:

The impact of service intensity on female participants is much more varied. In general, there is a consistent increase in net impact as the level of service is increased. Those female participants who gain access to OJT and PSE (Level 5) experience a net increase in annual earnings of at least $1,487, at least a twelve-week increase in employment per year, and at least a six-week reduction in welfare dependency. Naturally, the same caveats about potential losses in the post-subsidy period apply.

As was the case for males, we find that female participants who receive no WIN services actually experience a net decline in economic status. What is of interest here, though, is that even very low levels of WIN service apparently do yield substantive benefits to female participants, unlike the situation for males. Notice, for example, the net gains in status -- $ 231 - $ 361 in annual earnings, 2.6 - 3.5 weeks of employment — achieved by female participants who received little more than placement assistance. This suggests that women on welfare, many of whom have been out of the labor market for some time, are not familiar with job-finding techniques and can be aided significantly by WIN job development and placement.**

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* A similar finding was reported in Schiller, WIN Job Search, op. cit.

** These phenomena may be a reflection of racial discrimination in the labor market, which would constrain placement efforts and thus measured net impact. The degree, if any, to which WIN itself contributed to racial inequalities cannot be ascertained from the sample data.

There are only minor gains in earnings associated with the receipt of Level Three services, primarily educational classes (plus Level Two services). This result is somewhat unexpected, and may be explained by selection biases to be discussed below (Section VI E). It is also possible that educational services are simply not very effective unless part of a more comprehensive employability plan. In addition, many persons who left school prior to obtaining a degree perform poorly in remedial education classes. In some cases, they drop cut of WIN specifically because of adjustment to the class schedules.

We, also observe that institutional training (Level Four) yields impressive net benefits for female participants, ranging from $469 to $622 in earnings and from 4.2 to 5.6 weeks of employment.

Racial differences in the impact of various services also affect female WIN participants, but are not nearly so pronounced as they are among males. At Level Three (institutional training), white females attain a net, lower-bound gain of $537 (t=2.6), while black females gain $451 (2.5) OJT/PSE gains for white females are $1,641 (6.9), compared to only $1,357 (6.9) for black females.

D. The Importance of Work Experience

While our estimates of the impact of various WIN service levels are very suggestive for resource allocation decisions, they are incomplete in one important dimension, namely the pre-WIN work experience profiles of participants. Much earlier in this report (Table 3.6) we indicated that many WIN participants have very recent work experience while others have virtually none. In other words, there is considerable variation among participants (and registrants) in the extent of work experience, and thus in their presumed employability potential. What we seek to determine here is how much WIN is benefiting these two different types of participants.

Males:

The actual variation in the pre-WIN employment experience of male participants is summarized in Table 6.5, together with their post-WIN experience. The pre-WIN experience refers to employment in t-2, i.e., to the period 6-12 months prior to WIN entry. The post-WIN employment data is annualized on the basis of all available observations (an average of nine months).

Among the most salient observations in Table 6.5 is the fact that one third (34.9%) of the male participants had no recent (t-2) work experience. In view of the fact that disabled men are exempted from the WIN program, this suggests a prolonged period of unemployment. At the other extreme, we note that another third (29.8%) of the male participants were fully employed in the t-2 period, suggesting considerable work experience.

Reading down the first column of Table 6.5 provides a quick overview of WIN's success in placing individuals of different work experience. WIN was unable to find jobs for over half (53.1%) of those participants with no

Table 6.5 Pre-vs. Post WIN Employment Experiences of Male Participants

Pre-WIN Employment Experience*		Post-WIN	EMPLOYMENT EXPERIENCE*					
	None	1-12 Weeks	13-24 wks	25-36 wks	37-47 wks	48 wks	Total	Percentages
None	53.1%	9.6%	9.0%	2.8%	7.9%	17.5%	100% (177)	34.9%
1-12 wks	34.1%	14.6%	12.2%	7.3%	2.4%	29.3%	100% (41)	8.1%
13-24 wks	14.3%	19.0%	14.3%	9.5%	14.3%	28.6%	100% (63)	12.4%
25-36 wks	26.3%	5.3%	10.5%	18.4%	7.9%	31.6%	100% (38)	7.5%
37-47 wks	32.4%	10.8%	10.8%	5.4%	5.4%	35.1%	100% (37)	7.3%
48 wks	20.5%	10.6%	4.0%	13.2%	13.2%	38.4%	100% (151)	29.8%
					Total		507	100%

* Post-WIN employment is annualized on the basis of available observations (an average of nine months); pre-WIN experience is annualized on the basis of data from period t-2 (6-12 months prior to WIN entry).

recent pre-WIN employment experience. On the other hand, nearly eighty percent of those male participants with full-time, full-year pre-WIN experience were able to get jobs in the post-WIN period.*

The figures provided in Table 6.5 clearly confirm that it is easier to find jobs for individuals with extensive work experience than it is for individuals with little experience. But they do not really tell us anything about WIN's contribution to those placements. As our previous discussion (Section IIIB) of gross vs. net changes emphasized, it is also true that individuals with greater work experience will have an easier time finding jobs on their own, and thus that their "placement" may not represent anything attributable to WIN. To identify WIN's true impact on the employment status of participants of varying work experience, we must again resort to our multivariate model of net benefits. Table 6.6 depicts the results of such an effort, with the WIN participants stratified into two subgroups, those with no recent (t-2) employment experience and those with some.

Focusing first on the estimates of average net impact for 'served' male participants, it is apparent that the placement statistics cited above greatly distort WIN's net impact. Indeed, our results suggest that WIN has no statistica significant net impact on males with recent work experience, despite the fact that such individuals manifest a markably higher placement rate. This is due, of course, to the fact that similar males in the registrant pool find jobs with equal frequency, even in the absence of WIN services.

On the other hand, we observe that WIN has a very large ($604) and significant net impact on male participants with no recent work experience. This again contrasts sharply with impressions created by gross placements flows, as described in Table 6.5.

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* These figures are consistent with, though slightly lower than, the job entry statistics obtained from program records (Table 6.1).

These initial impressions of net impact are supported by observations on the different levels of service WIN provides. In particular, note that institutional training yields a very large ($1,346) and significant benefit to male participants with no recent work experience, and virtually no net benefit to more experienced males. This suggests that estimates of the average impact of vocational training for male participants is a poor generalization based on the experienced of very different types of participants.

Only OJT/PSE provides a demonstrable benefit ($1,579) to male participants with extensive work experience. Even here, however, the estimated net benefit is nearly twice as large for those who have had no work experience ($2,736).

Table 6.6 Estimated Net Impact of WIN, by Service Levels and Work Experience

	All Served Participants	One	Two	Three	Four	Five
Males with Recent Work Experience (N=672)	$190	-$1,629**	-$226	-$1,043	$ 116	$1,579***
Males without Recent Work Experience	$604**	-$810	$224	-$1,102	$1,346	$2,736***
Females with Recent Work Experience (N=1188)	$ 41.	-$730**	-$143	-$264	-$118	$1,188***
Females without Recent Work Experience (N=1823)	$676***	-$511***	$500***	$147	$840***	$1,710***

* Significant at .10 level
** Significant at .05 level
*** Significant at .01 level

Females

Our observations for male participants are mirrored in the experiences of female participants. From Table 6.7, note that two-thirds of the female participants have no recent (t-3) work experience, and that slightly over half (56.8%) of them fail to find employment in the immediate post-WIN period. On the other hand, only one-fourth (25.5%) of the female participants with full work experience (48 weeks) fail to find post-WIN employment. These differences again seem to imply that WIN has more impact on the most employment-ready in the target population.

These initial impressions are completely reversed when we turn to estimates of net impact, however, as detailed in Table 6.6. Beginning with average estimates for all served participants (an average of Levels Two-Five), we observe that females without recent work experience achieved a net gain of $676, while those with such experience gained a statistically insignificant $41 from their WIN participation. Furthermore, this kind of disparity in net impact is evident at every service level. Indeed, the net earnings gain to female participants with no recent experience is $500 if they received placement assistance, $840 if they received institutional training, and $1,710 if they received OJT/PSE. The overwhelming implication of these estimates is clear: WIN is much more effective, on a net basis, in serving those who have little or no recent employment experience, i.e., those who cannot and do not readily find jobs on their own.*

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* The subject of cost effectiveness is taken up in Chapter VIII.

Table 6.7 Pre-vs. Post WIN Employment Experiences of Females Participants

Pre-WIN Employment Experience*	Post-WIN	EMPLOYMENT EXPERIENCE*						
	None	1-12 Weeks	13-24 wks	25-36 wks	37-47 wks	48 wks	Total	Percentages
None	56.8%	6.2%	6.2%	6.5%	6.2%	18.1%	100% (1100)	66.0%
1-12 wks	33.3%	13.1%	16.7%	9.5%	4.8%	22.6%	100% (84)	5.0%
13-24 wks	37.3%	8.8%	7.8%	8.8%	12.7%	24.5%	100% (102)	6.1%
25-36 wks	41.2%	5.9%	5.9%	15.7%	3.9%	27.5%	100% (51)	3.1%
37-47 wks	34.1%	9.1%	4.5%	9.1%	11.4%	31.8%	100% (44)	2.6%
48 wks	25.5%	6.6%	10.8%	8.4%	10.5%	38.1%	100% (286)	17.2%
						Total	1667	100%

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* Post-WIN employment is annualized on the basis of available observations (an average of nine months); pre-WIN experience is annualized on the basis of data from the period t-2 (6-12 months prior to WIN entry).

E. Problems of Interpretation

All of our estimates of WIN service level impact, whether average or by client characteristics, are highly suggestive, but care must be taken in interpreting them. We have already noted the potential bias problem arising from the brevity of the post-WIN observation period and the fact that the net gains for OJT/PSE include continuing public subsidies. Here we would have to call attention to two other problems, one of potential bias and the other of policy inference.

In interpreting the impact estimates detailed in this chapter a basic conceptual problem must be recognized. Employment services are distributed largely on the basis of demonstrated need. As we observed in Section VI A, it is generally true that those with the least skills get skill training, while those who are job-ready get job-finding assistance. That is to say, the employment services are essentially remedial in nature, designed to bring all participants up to a certain level of job entry. In an ideal program in which the right service is always provided, and the service is always effective, each person would achieve some relatively comparable level of job entry, dependent only on his or her potential. As a result, those who had started out at least job-ready would manifest the greatest status improvement, and the services they received would be regarded as the most effective.

There are two problems here. First, one cannot conclude from such observations that everyone should receive some particular service just because it yields the greatest net benefit. Assume, for example, that basic education is observed to produce the greatest status gain. Clearly, not everyone will need basic educational services, and program resources would be wasted if universal distribution was attempted. If one were to observe that education services had the greatest impact on net status, the most one could conclude is that the program should increase its efforts to "recruit" registrants who need that service. Indeed, our last set of estimates, those that take account of pre-WIN work experience, seem to affirm this point.

The second problem arises from the implied selectivity in admissions policy. Local WIN managers are prone to select program entrants on the basis of their 'match' with available program components. Thus, individuals receiving services tend to be different from those not receiving services, and this creates estimation problems. Although our multivariate methodology attempts to control for such influences, it must be recognized that traditional demographic variables are very poor proxies for the need for supportive and manpower services. All of which means that our estimates of service impact, based on a nonexperimental design, must be treated as rough orders of magnitude.

VII. THE PROVISION OF SUPPORTIVE SERVICES

In the WIN concept, provision of supportive services by the Separate Administrative Unit (SAU) of the welfare agency is an integral part of the program. Many prospective participants need child care, transportation assistance, or other services tied to finding and keeping a job. Indeed, the extent to which such services are available frequently influences decisions about which registrants will be selected for program participation. This is particularly evident in the case of child care or transportation: those with service needs the program cannot meet will often be denied participation, regardless of other indications of employability.

The discussion of supportive services is organized as follows. First, the role of the site-level SAU's is presented. This is followed by a description of the processes by which the service needs of prospective WIN participants are assessed. Finally, each of the most frequently provided services is discussed, together with their actual distribution and participants' responses to them.

A. The Separate Administrative Units

Just as ES/WIN organizations varied from site to site, so did the SAU's. One variable was size: the SAU's at the sampled sites ranged from a single staff member to a dozen. At the time of the 1975 site visits, seven WIN operations had no SAU at all (down from eight sites in 1974). Three of these were sites where WIN was operated directly by the welfare agency, so that no discrete staff were designated as SAU. At two others, there were simply no services being provided. (For that matter, there were no new registrants being sent to ES/WIN.)* In the remaining two sites, WIN supportive services were provided by regular case workers.

At the sites that did have SAU's, the client-to-staff ratio varied enormously: from 1400-to-1 to 32-to-1. Nineteen sites experienced reductions in SAU staff size between 1974 and 1975; four sites had increases in staff, and at the remaining sites the staff level remained constant.

The client-staff ratios can be misleading, however, since not all reported SAU caseloads were composed exclusively of active WIN participants. In fact, only 30 sites had participant-only caseloads. At the others, SAU's also provided services to WIN registrants and to non-participating AFDC recipients. Often, the same caseworker handled a mix of WIN and non-WIN cases. Though this situation was more often a result of necessity than of planning, it frequently appeared to work to the WIN participant's advantage, since the worker was not isolated from "regular" welfare channels and was knowledgeable about all the service sources and opportunities known to the welfare agency.

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* These two sites were both in the same midwestern city; the situation reflected prolonged antagonism between the welfare department and WIN, which had led to the welfare agency's virtual refusal to have anything to do with the program.

While the SAU's were a relative novelty at many sites during 1974, by the time of the 1975 visits most of them had been thoroughly integrated into the WIN operation; the degree of cooperation between social service and manpower WIN elements improved strikingly from one year to the next. (One mark of this improved liaison, as noted in Chapter II, was the increased degree of colocation of the WIN manpower and service functions.)

Within the SAU's, the professional staff was usually composed of "generalists;" that is, caseworkers who arranged whatever services were needed and available. In 18 sites, though, there were specialists in some particular service -- usually child care -- on the SAU staff.

Besides service provision, the other key role of the SAU is participation in appraisal. In all but three of the sites with SAU's, the SAU performed joint appraisal with ES/WIN, or pre-appraisal apart from ES/WIN. In the majority of cases, the SAU's determination of the service needs of prospective participants was accepted without challenge by ES/WIN.

At the sites where the manpower and supportive service aspects of WIN have t on the identity of a single organization, division of labor has blurred considerably. (From the participant's perspective, there is no differentiation between ES/WIN and welfare staff -- all program employees are simply "WIN" personnel. At these sites, it is quite possible for a participant to go through the program without realizing that some of the staff are paid by the welfare department, and some by the employment service.) In such situations, SAU may be arranging services that are administratively the responsibility of the manpower agency: transportation to employment, and work-related expenses. SAU staff may also be providing vocational counseling, and even arranging placements.

Despite the overall trend toward effective manpower-welfare coordination, seven sites in 1975 were still experiencing significant friction between the SAU and ES/WIN. Typically, this involved differences of opinion about whether recommended services were really needed, and similar disputes about areas of authority and responsibility.

The other significant variation among SAU's was the degree to which they secured services by referral of participants to other agencies, versus providing them within the welfare agency or the SAU itself. This was less a matter of policy than of availability of resources; generally, SAU's tended to make use of whatever was available. About half the sites provided some significant portion of WIN supportive services by referral to agencies outside the welfare agency; however, at only 13 sites did SAU staff feel that available community resources were adequate to the needs of WIN clientele.

In sum, the trend is toward productive integration of the SAU into local WIN operations, including evident movement toward creating at least the appearance of a single, unified program.

A. 1 Service Assessment:

Typically, at some point in the participant's WIN experience an assessment is carried out by the SAU to determine service needs. At a few sites (nine), this assessment was carried out immediately after a prospective participant had filled out the AFDC income maintenance application. Thirteen sites performed assessment during pre-appraisal of prospective participants. Most of the remaining sites made the service assessment a part of the appraisal interview; however, a few sites conducted no service assessment unless a participant specifically requested some service.

The timing of the service assessment sometimes influenced the timing of the provision of 90-10 services.* Twenty sites began providing 90-10 services anytime after WIN registration, regardless of whether the registrant would ever become an active participant. Services provided between registration and participation usually consisted of transportation to the WIN office for appraisal, and -- in exceptional cases -- child care during WIN entry procedures (pre-appraisal, appraisal, job search). Three sites began 90-10 supportive services at pre-appraisal; the remainder provided services only to active program participants.

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* The term "90-10 services" refers to the federal-state funding formula. The Federal Government pays 90 percent of WIN service costs, but only 75 percent of regular AFDC services. Thus, the state dollar "buys" nine federal dollars in WIN, as against three federal dollars in other AFDC services. This situation, of course, creates an incentive for states to provide services from the WIN budget whenever possible.

Most of those sites providing services to individuals prior to active program participation focused their efforts on the most employable prospects. The theory was that if needed services could quickly be provided to essentially job-ready applicants, they might find work and not ever really enter WIN (or even the income maintenance rolls). *

The system of having a service assessment -- and provision of needed services -- prior to appraisal often worked well, significantly lowering the did-not-report rate for appraisals. In more than half the sites without such early assessment, failure to show up for appraisal interviews was a serious problem — and the cause often turned out to be lack of transportation or child care.

B. The Services Available

The services most commonly provided to WIN-II participants were child care and transportation assistance. These services, and the others provided at some sites, are discussed briefly below.

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* It should be noted that any net employment gains achieved as a result of such procedures would not be counted as "WIN impact" by our approach.

B. 1 Child Care:

In all but one site, SAU staff indicated that child care was a service frequently needed by WIN participants. (The exceptional site was one where the WIN population is virtually all male.) In general, though, child care is not the problem it once was in WIN, largely because many sites accept only those women who find their own child care arrangements.

The legislative provision that mandatory WIN registrants can be compelled to accept child care arrangements was either unknown or ignored at virtually all sites. Indeed, no case was encountered of anyone's being obliged to place children in a child care situation against the participant's wishes, though a few sites considered recalcitrant parents "non-cooperative," a term without legal or administrative meaning.

There was some growth in the use of child care units in SAU's -- 10 sites had such units in 1975, as compared with seven in 1974. Another 17 sites had child care units in the regular social services department, with those services available to WIN participants as well as other AFDC recipients.

Parallel with the trend toward more child care plans arranged by participants themselves (instead of by the SAU or other welfare instrumentality) was a trend toward relaxation of licensing or other day care approval requirements. In 1974, some 63 sites required formal agency approval of WIN child care plans; by 1975, the number was down to 53. SAU and other social services staff explained this trend was a result of time constraints (particularly where staff reductions had occured), as well as increasing requests from mothers that they be allowed to arrange their own child care.

In any event, most WIN child care plans were arranged by participants themselves, and most were babysitting arrangements. This type of child care was preferred by participants, but problems were reported, particularly in delay of vendor payments to babysitters, which resulted in a high turnover.

Where participants could not find their own child care arrangements, the ability of the SAU to fill the gap varied significantly. Fifteen sites reported adequate facilities and resources were available; the others had problems finding arrangements, or paying for them. Again, this problem was frequently "solved" by screening out persons with difficult child care problems.

Other problems with child care included:

- At many sites, the child care plan could not take effect until the participant's first day of employment, making it impossible to test the reliability of the arrangement in advance. Often, a plan that looked good on paper didn't work out, leading to the participant's failing to show up for work, or arriving late.
- Many prospective participants needed after-school care for their children, a particularly difficult arrangement to make. Only 11 sites were able to provide after-school child care for more than half the participants needing it.
- Transportation to day care centers and other providers often proved difficult to arrange, particularly in rural communities.
- In many areas, restrictive regulations existed. For example, relatives could not be paid as child care providers if they lived in the participant's home. And, allowable compensation for providers was less than generous -- some sites permitted a maximum reimbursement of $3.50 for up to 12 hours of care, out of which the provider had to supply a hot meal and two snacks. Approval requirements led to a kind of "Catch-22" in some communities: the WIN staff insisted that there be a stable child care plan before they would make a placement, but welfare regulations forbade approval of a child care plan until the mother was actually working.

B. 2 Transportation:

Transportation was the other service need often addressed by SAU staff. At some sites, transportation difficulties were far harder to solve than child care problems. In most areas, the problem could not be solved by providing tokens or passes for public transit, because public transportation was either nonexistent, or inadequate to link participants' homes to training or job sites. In fact, at 60 sites there was inadequate public transportation between participants' homes and welfare and WIN offices. Only 12 sites had adequate public transportation to major industrial areas.

The by-now-familiar exodus of manufacturers from core cities to suburban industrial parks has worked a particular hardship on low-income workers, including prospective WIN participants. For the inner city dweller, working at such distant locations demands a dependable car; rare indeed is the metropolitan area with convenient public transportation for "reverse commuters" -- those who travel from the city in the morning, and back in the evening.

Confronting this problem, WIN has offered other types of transportation assistance. In 1975, some 49 sites provided car repair money to participants (up to $300 at some sites), and 30 sites provided transportation allowances for taxis. Thirty-seven sites used vans or other vehicles to transport participants to training schools, day care centers, and jobs. Only three sites provided no transportation assistance.

Though car repair was the most widely used form of transportation assistance, this did not help the participant who did not have a car. At three sites, prospective participants who did not own cars were excluded from the program. While there was some experimentation in the early days of WIN with programs to assist participants in purchasing cars, none of the sampled sites were involved in such efforts by 1975.

B. 3 Other Services:

Once past child care and transportation, WIN supportive services thin out considerably. Other services encountered at some sites included medical examinations, remedial medication, family planning, homemaker services, housing services, and provision of uniforms and work equipment.

Medical examinations. Once universal in WIN, medical exams are now used selectively, in cases of need; e.g., when the applicant claims a medical exemption from the registration requirements, or a WIN or SAU worker suspects a medical problem, or a prospective employer requires an examination as a condition of employment.

Four sites were still providing exams to all, or virtually all, registrants as part of the program. Seven sites did not permit exams at all. The remaining sites made selective, and generally infrequent, use of exams.

Most sites (45) simply had persons use their own family doctors for examinations, with the cost generally borne by Medicaid or by the client. Other sites used public health departments, vocational rehabilitation agencies, public clinics, or doctors in the employ of the welfare agency.

Remedial medication. Where medical treatment was indicated, it was provided by the client's doctors (and generally paid for by Medicaid), or by rehabilitation agencies. WIN itself did not pay for remedial medication, nor did the program generally work with persons who needed any substantial degree of medical help.

Family planning. Some family planning services were available at nearly all sites (72), and 16 sites required that SAU staff discuss family planning with all WIN participants. Aside from counseling supplied by SAU or other welfare staff, services were provided on referral by Planned Parenthood clinics at 48 sites.

Homemaker services. Homemaker services were supplied by WIN (using 90-10 monies) in half the sites, but only 13 of these were providing services in most cases of need. Five sites used homemakers as child care providers.

Housing services. Helping participants with housing problems (landlord-tenant disputes, apartment-hunting, arranging repairs) was rarely found; however, half the sites did maintain lists of moderate-priced apartments for participants, or helped participants apply for public housing. There were a few instances of WIN helping with emergency minor home repairs (smashed windows, broken locks) by giving the participant money from a petty cash fund.

Work-related items. Uniforms for work were provided to WIN participants, by ES/WIN or SAU, at 45 sites. Tools were provided at 38 sites, and books (for education components or institutional training) at 25 sites. The provision of such items is dwindling. At some sites, staff complained that expensive tools and equipment had been lost when participants dropped out of training or work and failed to return them. In contrast, some WIN offices had rooms brimming with returned tools and uniforms that were not being used because participants didn't want used clothing, or the tools were too specialized to be of use in current training and employment situations.

C. The Distribution of Supportive Services

Although supportive services assumed an increasingly minor role as WIN-II increased its emphasis on job placement, we may inquire as to how many participants actually received some supportive service. We have also sought to determine the frequency of service receipt among comparison-group respondents, so that any advantages accruing to WIN participants might be identified. In tabulating service frequency we have included food stamps, Medicaid, and public housing, as they are important sources of real income to the AFDC population and may influence work behavior. Our tabulations are reported in Table 7.1.

Looking first at child care services, we observe that WIN participants received nearly all of the available WIN assistance, as relatively few individuals in the comparison group of nonparticipating registrants reported receiving such help. Those who did so, as noted in the foregoing section, were likely to have been highly employable and simply bypassed WIN certification, even though they obviously benefited from the WIN program. WIN transportation services appeared to be distributed much like child care services, with female participants receiving nearly all of this assistance.

Table 7.1

Receipt of Supportive Services, Food Stamps, and Public Housing, by Sex

	Males		Females	
Service	Participants	Comparisons	Participants	Comparisons
WIN Child Care	8.4%	0.8%	38.0%	6.5%
WIN Transportation	8.5%	1.0%	37.8%	6.5%
Food Stamps	79.3%	67.6%	83.8%	80.1%
Medicaid	90.2%	82.4%	95.5%	92.8%
Public Housing	23.8%	24.2%	32.6%	34.1%

Note: Figures refer to the percentage of respondents reporting receipt of each service.

With respect to in-kind forms of income support, it is evident that nearly all WIN participants and registrants use Medicaid and an extremely high proportion use food stamps. In addition, approximately one out of four male-headed families and one out of three female families live in public housing. Taken together, these various forms of income support serve to increase significantly the real incomes of the WIN population.

D. Participant Perspectives

There is considerable interest in the question of whether or not various services and in-kind supports influence the post-WIN employment and income status of WIN participants. However, such questions cannot be answered in a non-experimental study like the present one. At best, we could determine whether or not various services were associated with status change. Little could be concluded from such observations, however, as some services (e.g. child care, transportation) are provided only when employment is found -- and thus, a status change occurs -- while others (e.g. food stamps and public housing) -- might actually be cut back as a result of the same event. In this context, observed statistical correlations have little causal significance.

We can learn something about the utility of supportive services by soliciting the views of those who received them. In particular, we may ask whether or not the participants who received services felt they were useful, and also what kinds of service needs they continued to have. To this end, all sampled participants were asked to report which services were "most helpful" and those services which were needed, but not provided.

D. 1 Most Helpful Services:

At each of the three interviews undertaken for this study every WIN-II participant was asked to report the most helpful service he or she had received. The choice was not limited to supportive services, but rather encompassed all employment and supportive services, including financial assistance.* Thus, the responses solicited provide some perspective on the relative importance to WIN-II participants of supportive services in general, as well as information about the relative utility of specific services. A summary of participant responses obtained during the third wave of interviews is provided in Table 7.2.

One of the first things to note in Table 7.2 is the relatively high regard in which supportive services were held by WIN-II participants. Both males and females reported a major supportive service, rather than a major employment service as being the "most helpful" provided by welfare or WIN. In assessing these responses, however, it is important to recall that relatively few WIN-II participants received education or training services and thus had no opportunity to assess its utility. On the other hand, a substantial proportion of female participants received child care assistance and nearly all participants benefitted from Medicaid, if not special WIN health examinations.

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* The questionnaire asked: "Of all the services or help welfare/WIN has given yo
which service was the most helpful?" (See question 40 in the Wave III question

Table 7.2 Service Reported Most Helpful, by Sex and Race

	MALES		FEMALES	
	White	Black	White	Black
jor Supportive Services:				
Child Care	0.4%	0.7%	6.7%	7.0%
Transportation	0.9%	0.0%	1.4%	1.3%
Medical	27.0%	23.7%	32.0%	23.1%
Subtotal	28.3%	24.4%	40.1%	31.4%
jor Employment Services:				
Education	1.9%	1.0%	8.2%	4.0%
Training	3.6%	2.9%	8.6%	6.8%
Job Placement	15.9%	12.8%	6.2%	8.5%
Subtotal	21.4%	16.7%	23.0%	19.3%
nancial Aid	32.2%	24.8%	21.9%	25.6%
her and Unknown	18.1%	34.1%	15.0%	23.7%
Total	100 %	100 %	100 %	100 %
	(252)	(101)	(625)	(881)

urce: Wave III participant interviews

When we look at specific supportive services it is evident that medical assistance rates most highly among the major supportive services received. Whether this reflects simply extensive use of Medicaid or specially-arranged WIN medical examinations is not known.

D. 2 Unmet Service Needs:

Because the responses on "most helpful" service are constrained by the actual distribution of such services, it is difficult to base many policy judgements on them. Reports on services needed, but not provided, however, do not suffer from so many ambiguities, and thus may be of greater program interest. In particular, responses to the question about unmet service needs* may indicate where greater program efforts are required, at least for the select group of WIN-II registrants actually enrolled for WIN services. These responses are depicted in Table 7.3.

A salient observation in Table 7.3 is the fact that nearly 80 percent of those respondents who had completed WIN-II reported no unmet service needs, either supportive or employment. Optimistically interpreted, this suggests that WIN had achieved job readiness or employment for all participants. More realistically, it would seem to indicate that the program is meeting the expectations of those it selects to service, expectations perhaps conveyed in the appraisal and orientation components. In this regard, participants report being "satisfied" with their WIN experience by better than a two-to-one ratio.

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* The interview question was: "Have you felt that you needed any services from welfare or WIN that it did not provide you?"

Table 7.3 Reported Unmet Service Needs, by Sex and Race

	MALES		FEMALES	
	White	Black	White	Black
None Needed	81.3%	84.7%	79.4%	73.0%
Major Supportive Services:				
Child Care	0.2%	0.0%	1.9%	1.7%
Transportation	1.8%	0.8%	2.5%	2.7%
Medical	2.6%	0.8%	1.6%	1.2%
Subtotal	4.6%	1.6%	6.0%	5.6%
Major Employment Services:				
Education	0.6%	0.4%	0.8%	1.5%
Training	3.0%	1.2%	1.8%	3.5%
Job Placement	3.9%	4.4%	3.8%	5.5%
Subtotal	7.5%	6.0%	6.4%	10.5%
Financial Aid	2.6%	4.4%	2.9%	5.7%
Other and Unknown	4.0%	3.3%	5.3%	5.2%
Total	100% (541)	100% (248)	100% (1070)	100% (1464)

As for unmet service needs, both males and females report a slightly greater unmet need in major employment services than in supportive services. Among the needed supportive services, transportation ranks highest among female participants while medical assistance is reported most frequently by male participants. The differences, however, are too small to merit much significance.

In assessing these reports of unmet services needs (as well as reports of most helpful services) it is important to keep in mind the unique character of the WIN-II participant population. As discussed in Chapter II, the WIN-II participant population was explicitly selected an the basis of its greater employability potential; thus, it was likely to have fewer and different service needs then the larger welfare population which is the frequent subject of policy debates.

CHAPTER VIII. THE QUESTION OF COST EFFECTIVENESS

The net impact estimates provided in this report generally suggest that the WIN-II program generated a small net economic benefit to the average participant, and fairly substantial benefits to participants with little work experience or who were enrolled in certain components. Nevertheless, however important such gains might be to individual participants, we must still ask whether the program is "worthwhile" from a cost effectivenss perspective. The significance of this question is underscored by our finding that individuals who received the highest levels of service -- and thus contributed the most to program costs -- benefitted the most. If such benefits do not exceed program costs, then the presumption exists that there are more effective ways of providing the intended benefits.

The question of cost effectiveness, then, may be addressed from two perspectives. We may ask whether the entire program is "paying its way," i.e., whether total benefits exceed total costs. Or we may ask which particular program components or service levels yield the highest ratio of benefits to costs. The first kind of question is basic to decisions on whether to continue a program, while the latter kind of inquiry leads to greater understanding of how to manage available program resources. A major objective of this study, as stipulated in the original Request-for-Proposal, was to address the second question, that of relative cost effectiveness. Unfortunately, we discovered in the course of our efforts that the cost data available at the local project level is inadequate for the purposes of such as inquiry. Accordingly, the purposes of this chapter are to examine the questions of average and differential cost effectiveness, to describe available cost data, and to provide some very tentative observations on both cost effectiveness questions.

A. Average vs. Differential Cost Effectiveness

As suggested in the foregoing paragraph, there are two very distinct ways of approaching the question of cost effectiveness. In fact, there are really two questions to be addressed, not just one. These are: "Was the WIN-II program cost-effective?" and "Which WIN components provided the greatest cost effectiveness?" The first question relates to the average cost effectiveness of the program, the second to the differential cost effectiveness of its various components.

Although the questions of average and relative cost effectiveness bear a striking similarity to each other they typically involve very different measures and assumptions. The question of average cost effectiveness is usually resolved in a conventional investment/payback context. That is to say, the units of expenditure (dollar costs) are assumed to be identical to the units of benefit (dollar returns) and the question of average cost effectiveness is resolved by examining the balance between costs and returns. This approach is frequently referred to as "cost-benefit" analysis.

The question of differential cost effectiveness is unique in two respects. First, it focuses on program components rather than on the aggregate program effort. Second, and more importantly, it does not require so strict an assumption about the identity of expenditure and return units. Instead, the question of differential cost effectiveness can be resolved by examining the relationships of cost to any assigned program objective. For example, we may ask which WIN program components achieve the greatest number of placements per unit of cost, without explicitly assigning a dollar value to placements. Our interest here, then, is in the relative efficiency of program components in achieving a particular objective, rather than in determining whether or not the pursuit of that objective is "worthwhile" from a more traditional (average) cost-benefit perspective.

B. Measurement Problems

While both questions of cost effectiveness are easy to formulate, their resolution is complicated by a variety of measurement problems. Consider first the question of average cost effectiveness, wherein both dollar costs and dollar returns need to be specified. At the most general level, we have to determine which benefits and costs are relevant. On the benefit side we confront a number of very different dollar outcomes in WIN: reduction in welfare grants, increases in earnings, increases in family or individual income, increases in taxes, reduction in medicaid or food stamp costs, etc. Just because each of these outcomes is expressable in terms of dollars does not mean that all of them or any particular combination of them are appropriate measures of payback. At a minimum, we have to adopt a particular investor's perspective, e.g., that of the taxpayer. From the taxpayer's point of view only some of the dollar outcomes (e.g., reduction in welfare grant, increased income tax revenues) represent a true return on public expenditures.

Even after a particular investor perspective is adopted, serious measurement problems remain. On the benefit side, we have to recognize that returns will continue to accumulate over time, and thus that some rate of discount must be established (not to mention the actual time stream of benefits!) On the cost side we have to recognize that reported program costs may not accurately reflect taxpayer expenditures. For example, much of the costs of the WIN program go into staff salaries and thus a certain portion of reported program costs is returned in the form of federal, state, and local taxes. To the extent that program expenditures have a "multiplier effect" on the nation's output, they will generate still further tax revenues, which should be taken account of on either the cost or benefit side of the payback calculation. Finally, we must recognize that dollar costs may be incurred by taxpayers, but not WIN, when a participant is assigned to a CETA project for training or subsidized employment, and thus that reported dollar costs for WIN will understate the taxpayer's true investment in the program.

These measurement problems are relevant, though not quite so serious, for the question of differential cost effectiveness. Because any accepted program objective can be used to measure the relative returns of different program components, only the cost side generates difficult measurement problems for the analysis of differential cost effectiveness. The limitations of available cost data, however, are sufficient to preclude definitive resolution of even the question of differential cost effectiveness.

C. The Limitations of WIN Cost Data

In order to obtain cost data for local project activity, a cost questionnaire was prepared jointly by the study team and the national WIN Program Office (OWIN). The questionnaire asked for an accounting of all direct and indirect costs associated with specific WIN functions (e.g., orientation, OJT, etc.). The questionnaires, together with supporting letters and instructions, were sent to the sampled states by OWIN in the spring of 1975 and returned directly to us by the states. The response rate for this effort was 100 percent, with the last questionnaires received in September 1975.

The completed cost questionnaires were analyzed by the study team. As a result of that analysis, it was determined that the available data was internally inconsistent and not usable for the stated objectives of assessing either average or differential cost effectiveness. First of all, as noted in Chapter II, WIN has made increasing use of services and facilities provided by CETA and other government programs. As a consequence, the direct costs reported for WIN understate the true public costs of providing services to WIN clients. Moreover, because the extent of "budget expansion" varies tremendously across local WIN sites, it is virtually impossible to use reported direct WIN costs as an index of relative cost effectiveness for various WIN components and services.

A second problem inherent in official program cost data is the lack of reliable line accounting. Expenditures do not appear to be triggered by activity in any traditional accounting sense. The accumulation of expenditures seems simply a way to make the budget come out at the end of the year. The cost system is largely an arbitrary allocation of funds among categories regardless of the actual activity expected or experienced in these categories.

A third problem arises from the lack of correspondence between the budget and activity cycles. Participant carry-in and carry-out from one year to the next can distort the relationship between expenditure and activity. For example, it may be possible for 20 PSE contracts signed in December, 1974, to be expended in that year, but the activity reflected in the next.

These major limitations of official program data result in wild indices of local and national program costs. For example, our initial interrogation of cost data collected from all 78 sampled WIN sites led to "findings" such as these:

- at one site, the expenditure data shows a 56 percent decrease from 1974 to 1975 in OJT but the activity data shows a 50 percent increase. Another site shows no expenditures for PSE in 1975 but five new PSE placements. Another site showed an increase of 31 percent in PSE expenditures from 1974 to 1975 but the number of PSE placements declined from 28 to one. Similarly, many sites with extensive orientation components reported no expenditure for this category, nor any expenditure in another line item that could be assumed to be a proxy for orientation. Some sites with little counseling activity would have a large line item for counseling; some sites with a lot of counseling, a small line item.

- at one site $605 purchased .11 positions for employer and union services but only.002 positions for community service. Since it is hard to imagine that community service is 55 times more expensive than employer and union services, one is left trying to figure out what is actually being bought. Similarly when it takes $15,000 to purchase .34 positions to develop OJT contracts but only $1,209 to purchase .19 positions to develop PSE contracts one either has to reject the data or marvel at the difference in staff quality between the two activities. (In most sites, and in the one in the example, both activities are actually carried out by the same individual). As would be expected, across the sites the correspondence is even more meaningless. One site purchases 2.67 positions for placement for $9,833; another, located a few miles away in the same state, 1.26 positions for $13,303; and another, again in the same state, 1.57 positions for $12,120. (The last site also purchased .43 positions for job development for OJT for $13,772!)

Clearly, the cost data available on local projects is not of much value. If we were to try to develop cost per component estimates by dividing activity by cost, totally absurd figures would result. For example, at one site a counseling session would appear to cost more than ten times what it did at another even though our own site assessments showed the sites to be similar in their approach to counseling. Moreover, the estimates obtained by making the indicated divisions would not correspond at all to those "accepted " at the national level. WIN estimates exposure to the labor market at cost $6.00, orientation $19.00 and a direct placement $478 dollars. There is no way to rectify these estimates with the site cost data obtained from the states. Since we have no confidence in the local data sent to us, we would rather not develop estimates totally different from national estimates. Of course, the national estimates are also questionable to the extent that they rely on reported cost data.

D. Estimated Program Costs

In view of the inherent limitations of local WIN cost data, we have chosen to estimate average and differential WIN costs ourselves, using national WIN totals as benchmarks. Our estimates are hardly definitive, but should be reliable enough to yield some impression of WIN cost effectiveness

Our estimate of average WIN cost is based on the officially estimated program cost for FY 1975 of $350 million, of which $222 million was for employment services and $128 million for supportive services. During both 1974 and 1975 there were somewhat over 350,000 new participants, accounting for an average employment-service cost of about $634.*

Because most supportive service costs arise from child care, homemaking, or other services provided primarily to female participants, the average supportive-service cost for males and females needs to be estimated separately. We can reasonably assume that of the $128 million in total supportive service costs, close to $115 million (90%) is expended on the 245,000 female participants and the remaining $13 million on the 105,000 male participants, yielding average supportive service costs of $470 for female participants and $125 for male participants.

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*In both 1974 and 1975 there were actually well over 500,000 total participants available. The reason for choosing new rather than total participants is based on the observation that if participation figures do not change radically, in each year about $350 million is being expended and somewhat over 350,000 new participants being added, so that the average expended in any year can be related to the average new participation, with the effects of carry-in and carry-out costs being relatively insignificant. Although this inflates the cost of participation, other factors tend to deflate it. Most importantly, WIN draws increasingly on other programs (particularly CETA) for training and public employment, and in 1974-75, may have 'saved' $50 million in this way. With such offsetting factors, the estimate of $350 million for 'new' WIN expenditures seems reasonable.

Thus, the average total cost for male participants should be in the vicinity of $760 and for female participants, $1,104. Even considering the problems of estimation, and the fact that certain costs have been ignored (e.g., registration for those persons not in WIN), and that the participation figures are somewhat inflated by the unassigned participants (seven percent of the total), these estimates are adequate for our purposes.

In order to compute differential cost effectiveness, we also need estimates of cost for specific WIN components. Since the average person-year costs (provided by OWIN) for PSE are $6,912 for OJT $4,782, and for institutional training, $2,181, the average per-slot costs should be about $6,000 for PSE, $2,000 for OJT, $500 for institutional training.*

In view of the fact that total OJT/PSE costs are approximately $90 million while institutional training absorbs another $35 million, we can assume that all other employment services cost $97 million or about $280 per participant.** Using this estimate, together with those described above,

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* These are based on a per slot PSE duration of 10.5 months, for OJT of about 5 months, and for institutional training of around 3 months. These figures cannot, however, be reconciled with national estimates of 55,000 OJT or PSE slots costing $89 million. The national estimates would imply an average OJY/PSE cost of $1,600.

** We are assuming here that all participants receive lower-level services. To the extent that individuals who enter training or subsidized employment make less use of lower-level services we are underestimating the true cost of providing lower-level services and overestimating the cost of providing higher-level services. The differences are not likely to be great, however.

Table 8.1 Estimated WIN Costs, by Sex

	Male	Female
overall average	$ 760	$1,104
average not including training, OJT or PSE	$ 405	$ 750
average per participant in:		
training	$ 905	$1,250
OJT	$2,405	$2,750
PSE	$6,405	$6,750

Note: See text for derivation of these estimates.

E. Estimated Present Value of WIN Benefits

For the purpose of assessing the average cost effectiveness of WIN we will focus on only two measures of benefit, namely the net increase in earnings experienced by participants and the associated net reduction in welfare grants. These two measures reflect the perspectives of aggregate economic welfare and the taxpayer, respectively, but are by no means a complete representation of either (see Section VIII B above). Our estimates of first-year benefits for these two outcomes, as detailed in Chapter IV, are:*

	Males	Females
net earnings gain	$ 292 - $ 418	$ 412 - $ 553
net welfare reduction	$ 164	$ 106

We noted in Chapter IV that these estimated benefits may continue, at least in part, past the first year. Accordingly, we must estimate the level of future benefits and determine an appropriate rate of social discount for assessing their present value.

Our earlier estimates (Section IV E) of the decay rate appropriate to first-year WIN benefits were extremely tentative, but did suggest that the decay rate applicable to female earnings gains was low, possibly even zero. A decay rate of zero would be extraordinary, however -- though not impossible -- and probably should not be used unless and until longer-run observations are available to confirm its existence. For immediate purposes we prefer to use three alternative estimates of decay, namely .35, .19, and .10. The .19 rate is based on a study of post-MDTA experiences,[*] while the others represent high and low estimates around this benchmark.

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* The net earnings gains used here are for 'served' participants (Table 4.2), the net welfare reduction for all participants (Table 4.1).

In order to calculate the present value of future benefits we have chosen two alternative estimates of the real rate of social discount, namely 3 percent and 6 percent. The former was suggested by Dr. George Johnson at an ASPER working conference on this subject,[**] while the latter represents a reasonable upper bound.

The present value of WIN benefits is defined by the equation

$$\hat{B} = \frac{Bt1}{(d + r)}$$

where $\hat{B}$ refers to the present value of benefits, Bt1 to the first-year benefits, d to the decay rate and r to the social discount rate. Using this equation we have calculated the set of benefit estimates depicted in Table 8.2.

Clearly, the range of estimates for the present value of net earnings gains attributable to WIN is quite large, and the selection of any single estimate as the "best" must be motivated by assessments of the underlying

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[*] Edward C. Prescott and Thomas F. Cooley, Evaluating the Impact of MDTA Programs on Earnings, University of Pennsylvania, October 1972.

[**] February 1976.

Table 8.2 Alternative Present-Value Estimates of WIN Benefit

	Assumed Decay Rate		
	.10	.19	.35
A. Present Value of Net Earnings Gain for Males, at Discount Rates of:			
.03	$2,246 - $3,215	$1,327 - $1,900	$ 768 - $1,100
.06	$1,825 - $2,612	$1,168 - $1,672	$ 712 - $1,020
B. Present Value of Net Earnings Gain for Females, at Discount Rates of:			
.03	$3,169 - $4,254	$1,873 - $2,514	$1,084 - $1,455
.06	$2,575 - $3,456	$1,648 - $2,212	$1,005 - $1,349
C. Present Value of Net Welfare Savings for Males, at Discount Rates of:			
.03	$1,261	$745	$432
.06	$1,025	$656	$400
D. Present Value of Net Welfare Savings for Females, at Discount Rates of:			
.03	$815	$482	$279
.06	$663	$424	$259

values for first-year benefits, the decay rate, and the real rate of social discount. In view of the fact that our upper-bound estimates of first-year gains incorporate a known upward bias (see Section III B and Appendix B.3), we prefer to use them only in conjunction with high decay and discount rates. By the same token, our lower-bound estimates of first-year gains should probably be used with low decay and discount rates. By this reasoning and our own preference for the use of 19 percent as a low estimate for the rate of decay, an acceptable range for the present value of net earnings gains for males would be $1,020-$1,327, and for females $1,349-$1,873. We caution the reader, however, that these estimates are dependent on the assumptions we have made, and are "reasonable" only to the extent that the underlying assumptions are valid.

Our estimates of the present value of welfare savings are subject to similar qualifications. The fact that only one estimate is presented for each decay-discount rate combination does not imply greater precision, but rather reflects the fact that the available welfare grant data permitted only upper-bound estimates. Thus, in accordance with the foregoing discussion we would prefer to use $400 as the best estimate for male welfare savings and $259 as the best estimate of female welfare savings.*

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* Recall that the first-year estimates of welfare savings for males ($164) was itself statistically insignificant.

F. Estimates of Average Cost Effectiveness

The foregoing estimates of cost and benefit provide a reasonable even if somewhat cluttered basis for assessing WIN's average cost effectiveness. With respect to males, we noted that the average cost per participant was on the order of $760, while the present value of net earnings gains is probably in the range $1,020-$1,327. On this basis it would appear that the WIN program is economically cost-effective for males, as the resource cost of the program is exceeded by the resultant gain in output. The same conclusion appears to be warranted for female participants, as the present value of net earnings gains -- or, equivalently, net increments to output -- exceed direct program costs.*

From the narrower perspective of the taxpayer, however, the WIN program does not look so cost-effective. In particular, it appears that taxpayers are paying $760 to achieve a welfare savings of $400 for male participants and $1,104 to achieve an even smaller welfare savings ($259) for female participants. Even if we added in all other taxpayer costs and savings due to WIN, as described earlier, it is unlikely that the program would achieve cost effectiveness on the basis of a taxpayer's strict accounting. In view of this, we may suggest that taxpayers will regard the program as being "worthwhile" only if they attach a premium to the concept of partial self-support for welfare recipients (vs. the alternative of simply giving the poor all or some of the dollars expended on WIN).

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* This conclusion also depends on the assumption that displacement rates associated with WIN activity are negligible.

F.1 Variations by Client Profiles:

Before leaving the subject of average cost effectiveness, it is important to recall the tremendous disparities observed in first-year net impact for WIN participants of different types. In particular, we demonstrated in Section VI D that the pre-WIN work experience of participants was the critical determinant of their first-year net gains; that participants without recent employment gained substantially ($604 for males, $676 for females), while those with recent employment gained little ($190 for males, $41 for females) if anything.* The question not yet addressed is whether the superior gains achieved by the less job-ready are justified by the presumably higher costs of serving such individuals.

In order to determine accurately WIN's cost effectiveness in serving the least job-ready, we would have to allocate average program costs by client type, a hopelessly ambitious task in view of the nature of the underlying cost data and the variations in service intensity experienced by individuals. Indeed, a cursory review of the distribution of employment services (Table 6.1) suggests that we may not even be correct in assuming that WIN spends more money on the less job-ready, as such individuals are much less likely to participate in OJT or PSE, the most expensive WIN components. Accordingly, in order to assess WIN's average cost effectiveness in serving different kinds of clients we have to examine the costs and benefits associated with specific components, i.e., the subject of differential cost effectiveness.

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* Recall that the net impact estimates for experienced participants were statistically insignificant.

G. Estimated Differential Cost Effectiveness

In assessing the relative cost effectiveness of WIN components, we will consider only three major WIN service levels, namely placement services only (Level Two), vocational training (Level Four), and subsidized employment (Level Five), as these are the only three that yielded significant net benefits to any group of participants. We will also confine ourselves to one outcome, that of net earnings gains, as it is the only one that is substantial and because all others are correlated with it. Thus, the question we ask is whether placement services, institutional training, or OJT/PSE are most cost efficient in achieving net earnings gains for participants.

Our costs estimates for major WIN components were depicted in Table 8.1. For present purposes we will assume that the average costs of providing Level Two services (placement only) are $405 for males and $750 for females. Recall that these figures include supportive service costs ($125 for males, $470 for females), as well as the costs of all services below Level Four (thus appraisal, orientation, and education). Our estimates of average Level Four (training) costs ($905 for males, $1,250 for females) are similarly constructed. As for subsized employment costs, we have combined

OJT and PSE costs into a weighted average, with the weights determined by the actual distribution of such slots (see Table 6.1); this procedure yields average cost estimates for Level Five services of $3,365 for males and $3,950 for females.

To calculate the present value of earnings gains attributible to specific WIN services, we must again refer to estimated first-year gains and appropriate decay and discount rates. As described in Chapter VI, the estimated first-year gains were:

	Males	Females
Level Two (placement only)	-$ 83 - $ 18	$ 231 - $ 361
Level Four (training)	$ 569 - $ 772	$ 469 - $ 622
Level Five (OJT/PSE)	$1,823 - $1,975	$1,487 - $1,558

We have combined these with a three percent rate of social discount and four alternative decay rates to generate the set of present-value estimates depicted in Table 8.3.

In assessing the present value of benefits attributible to specific WIN services it is important to recognize that the same decay rate will not apply to all services. On the contrary, one would expect vocational training to provide some sort of permanent improvement in marketable skills and thus a lasting advantage to participants who are so served. By this reasoning, the decay rate associated with Level Four services (training) would be relatively low. On the other hand, placement services may provide only a short-run advantage to WIN participants, and thus manifest a high benefit

Table 8.3 Alternative Present-Value Estimates of Net Earnings Gains Due to Specific WIN Services

	ASSUMED DECAY RATE*				AVERAGE COST
for Male Participants in:	.10	.19	.35	.50	
Level Two (placement only)	0-$ 138	0-$ 82	0-$ 47	0-$ 34	$ 405
Level Four (training)	$ 4,377-$ 5,938	$2,586-$3,509	$1,497-$2,031	$1,073-$1,457	$ 905
Level Five (OJT/PSE)	$14,023-$15,192	$8,286-$8,977	$4,797-$5,197	$3,440-$3,726	$ 3,365
for Female Participants in:					
Level Two	$ 1,777-$ 2,777	$1,050-$1,641	$ 608-$ 950	$ 436-$ 681	$ 750
Level Four	$ 3,608-$ 4,785	$2,132-$2,827	$1,234-$1,637	$ 885-$1,174	$ 1,250
Level Five	$11,438-$11,985	$6,759-$7,082	$3,913-$4,100	$2,806-$2,940	$ 3,950

Note: A 3 percent rate of (real) social discount is assumed; zero values are noted where negative gains were recorded.

decay rate. Finally, we must realize that virtually nothing is known about the decay of observed Level Five benefits, as our observations have been restricted to the subsidy period.*

G.1 Males:

With all these caveats in mind, we may use Table 8.3 to formulate some conclusions about the relative cost effectiveness of different WIN service levels. The most striking observation is the failure of placement services to achieve economic cost effectiveness for males, even under the most generous assumptions about benefit decay. Indeed, even if we ignore the supportive service costs ($125) associated with the average male placement and assess placement activity only in terms of average employment service costs ($280), Level Two services would still fail to achieve cost effectiveness from an economic perspective.

By default, then, higher-level WIN services necessarily achieve a higher degree of cost effectiveness for male participants than do placement services. Among the higher level services we also observe that both vocational training and subsidized employment appear economically cost effective at all four decay rates considered. Under these circumstances it is difficult to determine which of these two service levels is more cost effective, particularly since we have no conclusive data on their respective decay rates. However, if Prescott and Cooley's findings about MDTA decay rates* are applicable to WIN and if the decay rate imposed on OJT/PSE gains at the end of the subsidy period is unusually high, vocational training would appear to be most cost effective. This conclusion is highly conjectural, however.

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* A recent study of MDTA found that the decay rate associated with OJT was higher than that for institutional training; see Cooley, McGuire, and Prescott, "The Impact of Manpower Training on Earnings: An Econometric Analysis" Carnegie-Mellon University, September 1975.

G. 2 Females:

The pattern of differential cost effectiveness associated with female participants is somewhat different from that observed for males. First, it appears that placement services for females may be economically cost effective, at least at lower rates of benefit decay. It is unlikely, however, that lower rates of benefit decay are actually attainable for such services.

The cost effectiveness relationships for vocational training are similar to those observed for placement services. Nevertheless, the extent that the first-year gains from training are easier to maintain over time than those from placement -- a most likely circumstance -- vocational training appears to offer superior cost effectiveness. The same may be said of OJT/PSE, as a strikingly similar pattern of benefit-cost relationships is apparent.

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* Ibid.

G. 3 The Importance of Work Experience:

Our observations on the average and differential cost effectiveness of various WIN services need to be extended to consider the effect of varying client profiles. As observed earlier, WIN participants with little recent jobs experience attain much larger net gains than more experienced participants. The actual differences in first-year earnings gains are (from Table 6.6):

Males in:	with recent experience	without recent experience
Level Two	- $ 226	$ 224
Level Four	$ 116	$1,346
Level Five	$1,579	$2,736
Females in:		
Level Two	- $ 143	$ 500
Level Four	- $ 118	$ 840
Level Five	$1,188	$1,710

On the basis of these first-year differences alone, some conclusive observations can be made. First, WIN placement services cannot possibly be cost effective for participants who have recent job experience as there are no net gains associated with such activity. Second, vocational training services, as presently delivered, are not cost effective for experienced females and attain cost effectiveness for experienced males only at very low decay rates.

In assessing the cost effectiveness of serving less experienced participants we must consider the possibility that such individuals are more expensive to serve. For convenience, we will assume that it costs twice as much to provide services to inexperienced participants at Levels Two and Four. At Level Two this means twice as many job referrals, twice as much education, etc. before a placement is achieved. At Level Four it suggests much longer training courses for the less experienced, in addition to the added education and placement efforts.

Using these assumptions we may calculate the likelihood of achieving cost effectiveness in serving less job-ready individuals. For simplicity we have calculated the maximum decay rate at which such activities would be economically cost effective. We find that placement services rendered to the less job-ready are economically cost effective at decay rates of 37 percent or less for males and 50 percent or less for females. While these rates indicate that Level Two services are much more cost effective when delivered to less experienced participants, these decay rates are probably not high enough to assure an excess of economic returns over resource costs. As we have discussed previously, it is likely that the net gains from placement services are very short-run in nature, as the passage of time increases the likelihood of comparable job-finding success by similar, nonserved individuals. Thus, we anticipate (but cannot document) very high decay rates for Level Two services.

Institutional training for less experienced participants clearly delivers an extremely high level of benefits relative to its cost. Indeed, for male WIN participants without recent employment experience institutional training costs are recovered in first-year net gains alone, even under the conservative cost assumptions we have stipulated. To the extent that such gains are maintained over time, such institutional training yields a multiple of economic returns over resource costs and thus appears to be the most cost-effective WIN service.

Subsidized employment also appears to yield a higher level of cost-effectiveness for less-experienced then more-experienced WIN participants. In reaching this judgment, we do not assume that the costs of OJT/PSE are higher for the less experienced. On the contrary, OJT and PSE subsidies are determined by the wage rate paid, and more qualified WIN participants are likely to get the higher-paying slots. Consequently, the only source of higher costs for the less job-ready resides in lower WIN service levels, and such differential costs probably do not significantly exceed the lower subsidies paid for the less job ready. Hence, we assume that the average costs of providing Level Five services are approximately the same for experienced and inexperienced participants ($3,365 for males and $3,950 for females). Under this assumption it is evident that serving the less job-ready participants yields greater cost effectiveness as their first-year earnings gains substantially exceed those of more experienced participants. Furthermore, the provision of OJT/PSE to male participants without recent employment experience attains long-run economic cost effectiveness so long as the annual decay rate does not exceed 81 percent! For females, the comparable "breakdown" decay rate is 43 percent.

IX. OVERVIEW AND CONCLUSIONS

The previous chapters have provided an array of observations and statistics about the structure and impact of WIN-II. So many, in fact, that only the most determined reader could have remembered them all, much less seen their interrelationships. Accordingly, in this final chapter we attempt to convey some salient impressions of WIN-II performance and impact, building on our earlier analysis. We do not attempt to recapitulate all of our earlier findings, rather just to formulate some conclusions.

A. The Elusive "Program"

Perhaps the most fundamental point to be made about the WIN-II program, as operated during the period 1973-75, is that it was not really a single, standardized program at all. As a result of continual changes in policy and program administration, resource constraints, and varying local needs and perspectives, the local WIN projects manifested tremendous variation in structure, goals, clientele, and performance. This evolutionary process is still underway, in fact, as local projects attempt to adapt to the WIN Redesign promulgated in July, 1975. At best, one can depict a national WIN-II program only in terms of the basic program concept and the kind of services and clients potentially available. Attempts to be more specific must inevitably come to terms with the inherent heterogeneity of the "program" referred to as WIN-II.

B. The Ambiguity of "Average Net Impact"

In the context of this program diversity estimates of "average net impact" are of uncertain value. The tremendous variation in local program structures raises the fundamental question of which "program" is having the observed impact. The question is further complicated by the diversity of services received by individual participants at any one site, much less the diversity across sites. Finally, in view of the negligible services actually received by many program "participants," the distinction between participants and non-participating registrants is extremely blurred.*

The basic ambiguity of average net impact estimates was illustrated in our own analysis by the observed sensitivity of such estimates to alternative definitions of participation and to client profiles. For convenience, we have summarized three sets of estimates in Table 9.1.

Our first set of average net impact estimates was based on the official distinction between participants and non-participants, and thus included as participants everyone who was certified into WIN; these estimates are noted as referring to "all participants." In general, they suggest that WIN-II had a very modest net impact on the earnings of male and female participants.

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* The WIN Redesign has formally eliminated this distinction, and is thus more compatible with the service-based evaluation methodology we have utilized.

Table 9.1 Alternative Estimates of Net Earnings Gains

Net Change in Earnings:	Males	Females
for all participants	$330*	$327**
for participants receiving any service	$292*	$412***
for served whites	$580**	$634***
for served blacks	$270	$255**
for participants with recent job experience	$190	$ 41
for participants without recent job experience	$604**	$676***

*	Significant at .10 level
**	Significant at .05 level
***	Significant at .01 level

Note: Only lower-bound estimates are depicted. The estimates are generated by our multivariate regression model, controlling for demographic characteristics, local unemployment conditions, levels of service, and site components.

Our second set of net impact estimates excluded from the participant group those individuals who were "unassigned" and therefore not provided with WIN services. Thus, we redefined "participation" as the receipt of some (any) WIN employment service. When gauged in terms of this concept of participation WIN's "average net impact" grew somewhat, at least for females.

Still greater variation in WIN's average net impact is evident when we consider the characteristics of the clients being served. The average earnings gains attributible to WIN are somewhat higher for females than males, twice as high for whites than blacks, and incomparably larger for inexperienced than experienced participants. Thus, the "average net impact" of WIN depends to a large degree on the kind of clients it chooses to serve. While it is always true that statistical averages conceal the diversity in their underlying distributions, the message of Table 9.1 is more than a statistical one. What is says is that WIN administrators have the power to alter measured average net impact simply by choosing the kind of client they wish to serve. It also suggests that estimates of average net impact should not be the sole measure of program performance.

C. The Importance of Specific Services

The ambiguities of "average net impact" highlight the importance of looking at the nature and level of services actually provided to WIN participants. Studies of individual services are also important in their own right, however, as they may provide suggestions for improved resource allocation within the program (and thus raise all estimates of average impact).

Our analysis of the specific services provided to WIN participants has suggested that there is tremendous variation in the efficacy of manpower services. Most striking of all is the extremely high level of net benefits accruing to participants in OJT or PSE. On the basis of the available evidence, it appears that most of the OJT subsidy and a substantial proportion of the PSE subsidy is directly benefiting WIN participants. That is to say, their net economic status appears to increase by an amount that approximates the OJT subsidy and a substantial proportion of the PSE subsidy. These "payoffs" to OJT and PSE have been measured over an extremely short period of time, however, so we know nothing about the status of such participants after their subsidies terminate. Hence, we cannot determine whether the training objectives -- as distinct from the income-support objectives -- of the OJT/PSE components have been fulfilled.

Other WIN employment services also appear to yield tangible benefits, but the size of such benefits is highly dependent on client profiles. Again we note that lower-level services have no net impact on participants with recent work experience, while both males and females who have little such experience benefit to some extent from training and other services.

While it appears that the shift in WIN resources from training and education to OJT and PSE, as mandated by the Talmadge Amendments, was most appropriate, a fundamental measurement problem must be repeated. We refer to the brevity of our "post-WIN" observation period. We have observed the post-WIN status of participants only months after they reported that their receipt of services (not necessarily subsidies) had ended. As a result, we not only are unable to measure long-run ("permanent") changes in status, but may end up with a distorted perspectibe on the relative pay-off of different services.

D. The Cost Effectiveness Message

Our analysis of the cost-benefit relationships in WIN generally confirms the implications of our net impact estimates. The principal message transmitted by the cost data is very clear: WIN is not economically cost effective in serving those who are likeliest to succeed on their own (i.e., those with recent employment experience), but attains reasonably high levels of cost effectiveness when it serves less experienced welfare recipients. This suggests that the gradual evolution of WIN into a "revolving door" program--in which the most job-ready are singled out for treatment and given quick placements -- has been misdirected and should be reversed.

Another message communicated by the cost data is that taxpayers cannot reasonably expect to recover the full costs of the WIN program in the form of reduced transfer payments. That is to say, average cost effectiveness is achieved only in an economic context, i.e., the net increments to output attributable to the WIN program exceed its resource costs. While this attainment benefits society as a whole, taxpayers as a class often assume a narrower perspective, weighing public expenditures against public receipts. From this perspective the WIN program is not cost effective and indeed could not be in view of the work incentive provisions built into the AFDC system. Accordingly, the WIN program will be regarded as "worthwhile" from this taxpayer perspective only if a dollar spent to encourage partial self-support among welfare recipients is less valued than a dollar spent on transfer payments.

E. The Unmeasured Deterrent Effect

Viewed from a broader perspective, our net impact and cost effectiveness estimates might be challenged on the basis that we failed to measure all of WIN's potential effects. As discussed in Section IVA, WIN may have two very different kinds of effects on the welfare population. On the one hand, WIN is designed to increase the welfare outflow by improving the net earnings and employment status of program participants. On the other hand, WIN may also affect the welfare inflow by making the application for welfare a more burdensome process (viz, the labor market exposure requirements). Because our study has focused only on the first kind of effect, it may have seriously understated WIN's complete impact on welfare roles and costs and distorted our impressions of differential cost effectiveness.

Although the data collected for this study cannot be used to estimate the size of the unmeasured "deterrent effect", we can gauge its potential significance by reviewing other studies. Most important in this regard is the experimental Community Work Experience Program (CWEP) implemented in California from 1972 to 1975. Under CWEP, eligible AFDC recipients were referred to nonpaying work experience assignments for a maximum of eighty hours per month. An explicit objective of these referrals was to "diminish the rate of the new welfare applications", i.e., to impose a substantial deterrent effect.*

While CWEP was never fully implemented, its performance provides some clues about the potential significance of the deterrent effect. Based on three years of experience, the state of California concluded that CWEP had no deterrent effect; indeed, that the rate of welfare applications actually rose more in counties with CWEP than in those that did not participate in experiment.** In view of this finding and the fact that WIN imposes much smaller costs on the welfare applicant that CWEP did, it seems reasonable to conclude that WIN's deterrent effect, if any, is small, and thus that our methodology for evaluating WIN does not seriously misrepresent WIN's true impact. Further support for this conclusion is contained in a study of the efficacy of work tests in stimulating job search and employment among Food Stamp and AFDC recipients in California, Texas, New York, Tennessee, and Nebraska: the principal conclusion of that study was that "a work test is not a powerful tool in encouraging work effort."***

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* See State of California, Third Year and Final Report on the Community Work Experience Program, April 1976.

** Ibid, p. 23.

*** Robert Evans, Jr., Barry L. Friedman, and Leonard Hausman, The Impact of Work Tests on the Employment Behavior of Welfare Recipients Brandeis University, May 1976, p. 85.

F. Bias, Bias, Bias

While the data provided in this study appear to support a variety of hard conclusions, we must recognize an element of uncertainty in our results. All of our estimates are plagued by one or more biases and we cannot be certain whether or not we have gauged WIN's true impact. While it is easier to bury all such problems in footnotes and then forget them, we believe that the constraints on our estimation procedures should be confronted directly.

The most important bias we have discussed arises from the shortness of our post-WIN observation period. Most of our observations span a period of only nine months after an individual has stopped receiving WIN employment services. From these observations we have estimated annual changes in earnings, employment, and welfare status. We have virtually no information on the critical issue of whether or not status improvements are maintained over longer periods of time. In addition, because our observations on the comparison group of non-participating registrants were timed to coincide with the "post-WIN" period, this truncation of our observations serves to exaggerate even the short-run gains attributed to WIN.

A second major source of potential bias emerges from the national recession which occured during our observation period. If the preliminary estimates of recessionary impact presented in Chapter V are valid, we must recognize that not only are our estimates of WIN's average net impact depressed, but also that our perspectives on the relative impact and differential cost-effectiveness of specific WIN services may be distorted.

Certainly, it is conceivable that the efficacy of placement services will be greater in better labor market settings, both absolutely and relative to other WIN services.

We also have to contend with various forms of selection bias in the WIN program itself. To the extent that WIN screens out the most difficult to employ, it misrepresents the true potential of employment services. To some extent these distortions can be controlled for in a multivariate estimation model, but such controls can never fully capture the effects of "creaming." Thus, selection bias becomes another potential source of misrepresentation.

Without recourse to a longer observation period or more information, there is little more than can be done about bias problems than to state them. Ultimately, the reader has to decide for him-or herself how the biases "balance out," and thus where the true impact of WIN lies. We have tried to facilitate this process by offering two sets of average net impact estimates (using different base periods), one with a known upward bias, the other with a known downward bias, and to focus on differences among client groups.

G. Future Directions

Subject to the data qualifications noted above, our study provides reasonably clear guidance for future program development. Most importantly, all available evidence indicates that WIN's increasing emphasis on the job-ready -- and the associated de facto exclusion of those AFDC recipients who are less employable -- has been misdirected. The basis for this conclusion lies in the observation that more job-ready AFDC recipients tend to find jobs as readily on their own as with WIN assistance, thus creating a situation wherein WIN has no net impact in serving the most employable. On the other hand, less job-ready AFDC recipients cannot and do not find employment on their own so readily, and thus benefit substantially from WIN activity. Hence, a larger share of WIN resources should be focused on those AFDC recipients with less employment experience.

In conjunction with this shift in intake priorities, the WIN program should also reallocate component priorities. There appears to be a solid case for resurrecting the employability-development components (especially vocational training) that were characteristic of early WIN efforts, while reducing the proportion of program resources allocated to job placement activities. In part, this conclusion is simply a logical extension of the first. In addition, however, it reflects the observation that regardless of client profiles,WIN attains greater net impact and cost-effectiveness from training and subsidized employment activities than from direct placement efforts.

While the appropriate direction for further WIN program development is reasonably easy to discern from the present evaluation effort, the distance to be traveled is less obvious. The complete abandonment of either placement activities or more job-ready AFDC recipients is certainly not advocated. Rather, we are concerned with a relative shift of priorities and resources. How much of a shift should be undertaken depends on a number of considerations we cannot fully explore. However, we should note three salient concerns: one of program scale, one of administrative responsibility, the third of labor market conditions.

With respect to the first concern, we do not know whether the returns to training, OJT, or PSE can be maintained at higher levels as such activities are expanded. Indeed, we do not even know the extent to which such activities can be expanded, given the constraints on local training facilities, employers' OJT needs, and public budgets. The feasibility and pay-off of larger-scale training efforts must be a continuing policy concern.

While we advocate an expansion of training activities for AFDC recipients, our data provide little guidance on the question of who should provide or administer such efforts. Many local WIN projects have apparently made good use of training, OJT, and PSE resources made available by local CETA sponsors, and we have no evidence to suggest that such coordination results in any greater or lesser effectiveness than efforts initiated by WIN itself.

The third constraint on the suggested shift of program resources and priorities emerges from our observation that WIN's effectiveness is sensitive to labor market conditions. We do not know whether or to what extent the impact of specific WIN services will rise when unemployment rates fall; under more favorable labor-market conditions, it is certain that placement activities will look better on a gross-flow basis, and might also produce some greater net impact.

Finally, we should note that if any of the above program shifts are to be pursued with vigor, a basic change in management perspectives is required, particularly with respect to the measurement of program performance. At present, gross placement rates are regarded as the prime criterion of WIN program success. These are an inadequate and misleading index of program impact, as we have demonstrated. Furthermore, if the structural changes suggested here are undertaken, gross placement rates will surely fall (although net impact will rise). Thus, program administrators are likely to resist suggested changes unless alternative indices of performance are introduced and emphasized.

TECHNICAL APPENDICES

Appendix A. THE REGRESSION MODEL

All of the estimates of WIN impact described in the body of this report were generated by some version of our basic multivariate regression model. The purpose of this appendix is to describe that model more fully

A.1 The Basic Model

The general objective of our methodology is to isolate the net impact of WIN participation on various dimensions of participant status. For this purpose, a basic multivariate model was defined as:

$$(1) \qquad Y_i = F(p_i, X_i, e_i).$$

The linear estimating equation corresponding to (1) is:

$$(2) \qquad Y_i = \alpha + \beta_1 p_i + \beta_2 X_i + e_i,$$

where Y = dependent variable

p_i = dummy variable taking the value 1 if an individual is a participant in the program; 0 otherwise

X_i = vector of independent variables denoting characteristics of individual i

α = constant term

β_1 = regression coefficient corresponding to p_i, WIN participation

$\beta_2 \ldots \beta_n$ = regression coefficients for variables in the X-vector

e_i = error term.

This model implicitly assumes that e_i is a normally distributed error term with expected value of 0 and a constant variance, σ^2. These error terms are further assumed to be nonautoregressive.

Four dependent variables (Y_i) were estimated:

- Change in annual earnings from pre to post WIN period
- Change in annual weeks worked from pre to post WIN period
- Change in annual weeks on welfare from pre to post WIN period
- Change in annual welfare grant from pre to post WIN period.

The X-vector of independent variables was defined as follows:

X_1 = 1 if $26 \leq$ age ≤ 39; 0 otherwise
X_2 = 1 if $40 \leq$ age; 0 otherwise
X_3 = 1 if education level is 9-11 years; 0 otherwise
X_4 = 1 if education level is ≥ 12 years; 0 otherwise
X_5 = 1 if race is white; 0 otherwise
X_6 = 1 if another wage earner was present in the household at Wave I; 0 otherwise

X_7=1 if there were at least one child under 6 years of age in the household at Wave I; 0 otherwise

X_8=1 if client participated in WIN; 0 otherwise (comparison group)

X_9=earnings in period t-1

X_{10}=earnings in period t-2

Note that X_8 refers to p_i, the dummy WIN participation variable, in equations (1) and (2).

The list of independent variables given above was used to predict earnings in period t+1 from earnings in periods t-1, t-2 or t-3, as explained in Section III C of the report. For the regressions forecasting change in the number of weeks on welfare, change in the number of weeks worked, and change in the annual welfare grant, variables X_9 and X_{10} above would, of course, represent the appropriate lagged economic variable. Variables X_1 through X_8 are the same for all regressions.

Variables X_1 through X_7 are demographic in nature. Prior studies on employability program research suggest that these factors are likely to be statistically correlated with the outcome variables.

A.2 Extensions of the Basic Model

The basic model defined by equation (2) treats WIN participation as a dichotomous event, i.e., people are either "participants" or "non-participants" according to the value of X_8. As such it is best suited for very aggregated estimates of net average impact. Extensions of this model were undertaken to improve on the definition of "participant" as well as to identify the impact of alternative service levels and components.

Our first extension of the basic model, then, entails the redefinition of "participant" (X_8) as follows:

X_{8u}=1 if "unassigned" participant; 0 otherwise

X_{8a}=1 if "assigned" participant; 0 otherwise

The regression is coefficient on X_{8a} is used as our estimate of net impact for 'served' participants, as discussed in section IV B of the report.

Further extensions of the basic model focused on the actual levels and kinds of services received by participants. In particular, three additional subsets of variables were defined and added to the basic model in the course of the analysis. These were:

(a) Levels of Service

Five levels of service, ranging from One (unassigned participant) to Five (OJT/PSE), to which every participant was assigned. These service levels substituted for the simpler distinction between X_{8u} and X_{8a} in the later stages of the analysis (section V B).

(b) Job Search Levels

Four levels of job search were defined, again as an alternative means of classifying participants. Job search level One is basically a combination of service levels Three thru Five, plus those at level Two who received "significant" job development and placement assistance (see Section V C). This definition of "full participant status" was used to generate the impact estimates of section V C, the ones regarded by the research team as the best single indices of WIN performance.

(c) Site Quality Variables

Fifteen site quality variables were added to the basic model in order to convey the significance of site-specific program characteristics. These variables were discussed in Section VI B of the report, and treated as complements rather then substitutes for individual-specific service variables.

We also added a measure of local labor market conditions to guage the independent influence of this factor on WIN impact. Specifically, we defined three states of change in local area unemployment conditions, as follows:

Low Change: less than 37 percent increase in local unemployment rate between Spring 1974 and Spring 1975.

Moderate Change: percentage increase ranging from 37% to 62%.

High Change: percentage increase greater than 62%.

This classification of unemployment changes was based on an approximate three-way partitioning of the site sample, according to actual unemployment experiences during the period Spring 1974 to Spring 1975. The actual experiences of the sites are described in Table G.1 .

Finally, we added a measure of the site response rate, in continuous form. This measure reflects the percentage of completed Wave III interviews at a local site, as a percentage of the initial (Wave I) interview sample. This variable is intended to capture the effects of nonresponse bias, at least within the range of actual response-rate variation.

The complete catalogue of variables used in our analysis is summarized in Table A.1. We used different combinations of these variables at various stages of the analysis, according to the issue being examined. In all, we ran eleven different combinations for each of three dependent variables, for each sex, for two different base periods for a total of 132 regressions.

Table A.1 Catalogue of Variables

Dependent Variables:

a. Change in Annual Earnings
b. Change in Weeks Worked (per year)
c. Change in Weeks on Welfare (per year)

Independent Variables:

1. Age 26-39
2. Age ≥ 40
3. Education 9-11
4. Education ≥ 12
5. Race (1=white)
6. Presence of other wage earner at Wave I
7. Presence of child under 6 at Wave I
8. Moderate unemployment change
9. High unemployment change
10. WIN Service level 1 (unassigned participants)
11. WIN Service level 2 (low level of services: other than levels 3-5)
12. WIN Service level 3 (education: 06,07,08 or 22)
13. WIN Service level 4 (training: 04 or 05)
14. WIN Service level 5 (OJT/PSE: 11,12, or 13)
15. ES controls appraisal services & manpower (0303/3) and Employability is used at appraisals to determine participation (0309.1/1)
16. Unknown how appraisal control is exercised (0303/8 or 9)
17. Site maintains a formal unassigned participant pool (0401/1)
18. Unknown whether unassigned participant pool used (0401/9)
19. Stop status is of such a duration or frequency that it impedes progress of the client (0601/1)
20. Unknown whether stop status impedes progress of the client (0601/9)
21. Counseling is extensively used, (1001/4)
22. Institutional training more than minor part or good choice of skills (1101/2 or 3
23. Institutional training, limited availability (1103/1 or 2)
24. Institutional training, N.A. (1103/8)
25. Little or no retroactive placements (1914/0 or 1)
26. Unknown whether retroactive placements used (1914/8 or 9)
27. Little or no manpower resources available (2005/0 or 1)
28. Little or no OJT and PSE (1401/0 and 1501/0)
29. OJT and PSE considered as major option (1401/3 and 1501/3)
30. Job Search level 1 (not used)
31. Job Search level 2 (informal)
32. Job Search level 3 (formal)
33. Job Search level 4 (other)
34. Site response rate
35. Dependent variable in prior period*
36. Dependent variable in period before prior period*
37. Dependent variable in regression***
38. WIN service level 2 through 5

A.3 The Site Controlled Model

While the Basic Model and its extensions satisfy most of our evaluation needs, it does have potential defects. Most importantly, it is quite possible that even our most complete specification does not capture all of the sources of performance variation across sites. When this possibility is combined with variations across sites in interview response rates, it raises the possibility of estimation bias. Although our site response variable (number 34) should reflect this bias, it is worthwhile to explore alternative methodologies for cleansing our impact estimates of potential site bias. To this end, an alternative to the Basic Model was specified in which all participant/comparison - group comparisons are made at specific sites - rather than pooled across sites - than averaged. Specifically, the following site-controlled model was developed and used for our initial estimates of net WIN impact (Section IV A):

$$Y_{ij} - \bar{Y}_{ij} = \alpha + \beta_1 (p_{ij} - \bar{p}_j) + \beta_2 (X_{ij} - \bar{X}_j) + \epsilon_{ij} \qquad (3)$$

where

Y_{ij} = the dependent variable for client i at site j,

$\bar{Y}_{ij}$ = the average value for variable Y_{ij} at site j

p_{ij} = dummy variable (for client i at site j) taking the value 1 if the client is a participant in the program; 0 otherwise,

$\bar{p}_j$ = the average value for the p_{ij} at site j. This actually represents the fraction of participants in the sample at site j,

X_{ij} = vector of independent variables denoting characteristics of client i at site j,

$\bar{X}_j$ = the average value for variable X_{ij} at site j,

α = constant term

β_1 = regression coefficient corresponding to p_{ij}, WIN participation,

$\beta_2 \ldots \beta_n$ = regression coefficients for variables in the X-vector, and

ϵ_{ij} = error term.

This model implicitly assumes that ϵ_{ij} is a normally distributed error term with expected value of 0 and a constant variance, σ^2. These error terms are further assumed to be nonautoregressive.

As in Section A.1, four dependent variables (Y_i) were estimated:

- Change in annual earnings from pre to post WIN period
- Change in annual weeks worked from pre to post WIN period
- Change in annual weeks on welfare from pre to post WIN period
- Change in annual welfare grant from pre to post WIN period.

The X-vector of independent variables is also defined in Section A.1.

It can be shown that the regression coefficients β_1 and β_2 in equation (3) above are equal to the regression coefficients β_1 and β_2 below tha would be obtained in the following model.

$$(4) \qquad Y_i = \alpha_0^* + \beta_1 p_i + \beta_2 X_i + \beta_3 Z_i + \epsilon_i,$$

where

Y_i = dependent variable,

p_i = dummy variable for WIN participation,

X_i = vector of independent variables,

Z_i = vector of dummy variables for each of the 78 WIN sites,

α_0^* = constant term, and

$\beta_1, \beta_2, \beta_3$ = regression coefficients.

Equation (4) is a model which controls for site variations explicitly by including a dummy variable for each site in the vector of independent variables. Equation (4), although correct, is not computationally feasible due to the many independent variables. Equation (3), which measures each variable, both dependent and independent, as a deviation from a site mean, and then regresses the corrected dependent variable on the corrected independent variable, is much easier to estimate. That equation (3) actually is equivalent to equation (4) is not immediately obvious, and a proof will be outlined below.

Outline of a proof that model(3) is equivalent to model (4):

This section outlines a proof that model (3) above is equivalent to model (4). To put the problem in general terms, let

$$Y = X_1\beta_1 + X_2\beta_2 + \epsilon \tag{5}$$

be a model including the site dummy variables where

X_1 is the vector of site dummy regressors,

X_2 is the vector of all other regressors (including the WIN participation variable).

Now let b_1 be the least squares estimate of β_1 and b_2 be the least squares estimate of β_2 in (5). It can be shown[3] that

$$b_1 = (X_1'X_1)^{-1}X_1'Y - (X_1'X_1)^{-1}X_1'X_2(X_2'M_1X_2)^{-1}X_2'M_1Y$$

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[3] Arthur S. Goldberger, Econometric Theory (New York: John Wiley and Sons, Inc., 1964), pp. 194-196.

and

$$b_2 = (X_2' M_1 X_2)^{-1} (X_2' M_1 Y)$$

where X' is the transpose of vector X and $M_1 = I - X_1 (X_1' X_1)^{-1} X_1'$, and I is the identity matrix. It is also true[4] that matrix M_1 is idempotent (a matrix A is idempotent if and only if A' = A and AA = A).

Now let

$\tilde{Y}$ be the residuals resulting from regressing Y and X_1 alone,

$\tilde{X}_2$ be the residuals resulting from regression each variable in X_2 on X_1,

$\tilde{b}_1$ be the estimate of β_1 obtained by regressing Y on X_1 alone.

The two models may now be presented as:

(6) $$Y = X_1 b_1 + X_2 b_2 + \epsilon$$

(7) $$\tilde{Y} = \tilde{X}_2 \tilde{b}_2 + \tilde{\epsilon}$$

It will now be shown that $\tilde{b}_2 = b_2$.

$$\tilde{b}_2 = (\tilde{X}_2' \tilde{X}_2)^{-1} \tilde{X}_2' \tilde{Y}$$

$$= (X_2' M_1' M_1 X_2)^{-1} X_2' M_1 M_1 Y \text{ (since } \tilde{X}_2 = M_1 X_2 \text{ and } \tilde{Y} = M_1 Y)$$

$$= (X_2' M_1 X_2)^{-1} X_2' M_1 Y \text{ (since } M_1 \text{ is idempotent)}$$

$$= b_2.$$

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[4] Ibid.

We now have shown that the full ordinary least squares estimate of b_2 in (6) can be obtained by regressing Y on $X_2 - X_1 B$ where B is a set of auxiliary regressions of each variable in X_2 on all the variables in X_1. Since the dummy site vectors in X_1 are orthogonal, the estimates of the auxiliary regressions may be obtained from simple regression alone. Furthermore, the simple regression estimate of a variable on the site dummy is just the mean value of the variable at the given site.

The above completes an outline of the proof that models (3) and (4) in the preceding section yield the same coefficient vector for the X_{ij}.

The standard errors of the regression coefficients of b_2 are also the same for models (6) and (7).

First, write (7) as

$$Y = X_1 \tilde{b}_1 + \tilde{X}_2 b_2 + \tilde{\epsilon} \tag{8}$$

$$\epsilon - \tilde{\epsilon} = X_1(\tilde{b}_1 - b_1) + (\tilde{X}_2 - X_2) b_2$$

$$= X_1(\tilde{b}_1 - b_1) + (M_1 - I) X_2 b_2 \quad (\text{since } \tilde{X}_2 = M_1 X_2)$$

$$= X_1(\tilde{b}_1 - b_1) - X_1 (X_1' X_1)^{-1} X_1' X_2 b_2$$

$$= X_1[\tilde{b}_1 - b_1 - (X_1' X_1)^{-1} X_1' X_2 b_2]$$

$$= X_1[(X_1' X_1)^{-1} X_1' Y - (X_1' X_1)^{-1} X_1' Y$$

$$+ (X_1' X_1)^{-1} X_1' X_2 (X_2' M_1 X_2)^{-1} X_2' M_1 Y$$

$$- (X_1' X_1)^{-1} X_1' X_2 (X_2' M_1 X_2)^{-1} X_2' M_1 Y]$$

$$= X_1[0] = 0.$$

This shows that the error vectors, ϵ and $\tilde{\epsilon}$, are equal.

The standard errors of the regression coefficients b_i are

$$s_i = \frac{\sqrt{V_{ii}} \cdot E}{n_d}$$

where V_{ii} is the i^{th} diagonal element of $(X'X)^{-1}$,

$E = \sum_{j=1}^{m} \epsilon_j^2$, and

n_d is the number of degrees of freedom in the regression.

The number of degrees of freedom is really the same in (6) and (7). Even though (7) has 78 more degrees of freedom than (6), those 78 degrees of freedom are lost since $\tilde{Y}$ is the result of regressing Y on the 78 site dummy variables.

Also, the partitioned inverse of the diagonal elements of X_2 in (6) is

$$V_2 = (X_2' M_1 X_2)^{-1}.$$

The inverse in (7) is

$$\tilde{V}_2 = (\tilde{X}_2' \tilde{X}_2)^{-1}$$

$$= (X_2' M_1' M_1 X_2)^{-1} \text{ (since } \tilde{X}_2 = M_1 X_2)$$

$$= (X_2' M_1 X_2)^{-1} \text{ (since } M_1 \text{ is idempotent)}$$

$$= V_2.$$

The outline of the proof that models (3) and (4) are equivalent is now complete.

Appendix B: KEY DEFINITIONS AND ASSUMPTIONS

Before applying our regression models to the interview data several key assumptions and definitions had to be formulated. Most importantly, we had to define WIN "participation" in a longitudinal context, choose an appropriate time frame for measuring changes in the dependent variables, and assess the appropriateness of the comparison group. Our decisions on these issues are explained here.

B.1 WIN Participant Definition

The most basic problem in the entire analysis was the division of the sample into two groups--the participant group and the (untreated) comparison group. For each individual WIN registrant sampled there were three sources of data on which to base this division:

(1) the original sample classification, based on official program files at each WIN site (Spring 1974);

(2) the three waves of WIN client interview questionnaires; (ending Fall 1975)

(3) the WIN record data gathered from program files (Summer 1975)

By the time the three waves of interviews were completed, the original sample classification no longer carried any useful information for defining participation, in view of the fact that many registrants moved into participation status over time. Indeed, over 30 percent of the original registrants had moved into WIN by Wave III and were thus bonafide WIN participants. Because of this movement, longitudinal data from the client interviews were given precedence over the original sample classification. WIN clients are classified as WIN participants if they responded affirmatively to the appropriate questions concerning their participant status. In particular, the following questions were used to determine whether an individual respondent was classified as a participant:

Wave I - Questions (1), (2), (2a), (7)
Wave II - Questions (1), (2), (2a), (3)
Wave III - Questions (1), (1a), (2), (2a), (3).

These questions ask respondents if they have ever participated in the WIN program, whether they are still participating, how long they have participated, and, if they have left WIN, how long ago they have done so.

Where a respondent maintains that s/he did not participate in WIN during the longitudinal study time frame, but the record data clearly indicates that the client has in fact participated, the client is classified as a participant for our analysis. These classification criteria can be generalized into a simple rule: A client is a participant if evidence of participation exists in any of the questionnaires or in the program record data.

One exception exists to the above rule. If checks of the participant data against the Job Entry date in the record data show that a WIN registrant moved directly into Job Entry, s/he is assumed to already have been employed at the time of WIN entry and a probable retroactive certification. These probable retroactive certifications have been excluded from the participant group and moved into the comparison group.

The primary argument for placing them in the comparison group is that they should be treated the same as any other (unserved) WIN registrants w ho have obtained jobs on their own. Eliminating this group from the analysis altogether would systematically reduce the earnings level of the comparison group. But the actual percentage of clients identified as probable retroactive certifications is only two or three percent, so the net impact estimates are not statistically sensitive to how they are classified. The problem of the "unassigned participant" does not arise in this particular categorization, as no level of WIN service is required for the basic definition of WIN participant.

B.2 The Pre-and Post WIN Time Frames

Having divided the sample into participants and nonparticipanting registrants (the comparison group), our next problem was to select appropriate time intervals for comparing their socioeconomic status (and this for guaging the net impact of WIN). This problem is both one of the trickiest and most fundamental in our net impact analysis. The analysis is designed to measure the economic impact of WIN as compared to a similar control group which was eligible for the WIN program, but did not actually participate. In principle, all one need do is compute the economic variables after WIN participation (post-WIN) and before WIN participation (pre-WIN) for participants, do likewise for the comparison group, and then compare one group against the other. However, there are many practical problems that must first be tackled; for example, one must determine the "pre" period and the "post" period for WIN participant and comparison groups. These periods are event-oriented for the WIN participants and follow naturally from their entrance into, and termination from, the WIN program during the longitudinal observation period. The "pre" observation period for WIN participants is the 24-month period immediately preceding WIN participation. This 24-month period is broken down into four 6-month periods and labeled t-1,t-2,t-3, and t-4, with t-1 denoting the most recent interval prior to participation (see Figure A-1). The "post" observation period is measured from the month in which the person terminated WIN to the last available month of data recorded in the Wave III interview, and labeled t+1. This is relatively straightforward. One problem which arises, however, is that in some cases the exact WIN termination date is unknown.

Where uncertainties arose, the WIN record data was consulted for a termination date. If no record data exist for the case, an operational definition which assumes the termination date to be one month after the Wave I interview is used.

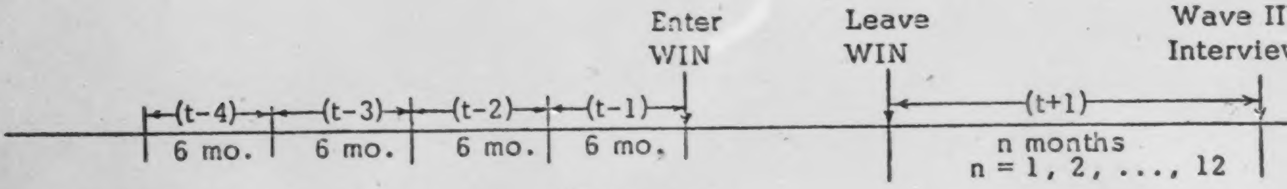

FIGURE A-2

Time Reference for WIN Participant

The unique nature of the event-oriented, survey-based definition of WIN termination used here should be noted. Officially, WIN participants are often regarded as still being "in the program" even after they are placed in a job. That is, Job Entry is frequently regarded as a dimension of WIN participation, and may last for ninety days. In such cases, a person is said to have terminated participation only after the Job Entry period is completed (thus, a CJE, or (completed Job Entry). By our definition, however, an individual is terminated--enters the "post-WIN" period--as soon as the provision of employment services ceases; in this case, when the initial job (entry) placement is made.

As noted in the text, our event-oriented definition of the post-WIN period introduces special problems in measuring the impact of OJT or PSE. From a client's prospective, a person is no longer "in WIN" once s/he is placed into an OJT or PSE slot. Indeed, clients are seldom told that their OJT/PSE job is being subsidized, and in any case, tend to regard it as any other form of employment. Accordingly, the "post-WIN" period for OJT/PSE placements typically includes the time frame (6-12 months) in which there employment is being subsidized. The short length of our observation period does not permit status measurements in the "post-subsidy" period.

There exists no natural method for constructing a time frame for the comparison group. Although it would be desirable to obtain measurements for both groups for exactly the same calendar periods, it was not possible to do so for this analysis. The alternative of fixing the "pre" period for both groups, in order to attempt to have a common calendar base from which to measure, was considered but rejected. The strongest argument against this alternative is that, for WIN participants, the time from the end of the "pre" period to actual entrance into the WIN program would vary from client to client. We know from the analysis of the comparison group bias (see below) that the period immediately preceding WIN participation is critical. Fixing the "pre" period to the calendar could vary the "waiting times" from t-1 to t from about 1 to 18 months, so the concept of fixing the base period was rejected. A second alternative was selected as the best available, given the constraints imposed by the study design and the present analysis. In this alternative, the "pre" period for the comparison group terminates with the average month in which clients entered WIN for each site. This average month marks the end of the "pre" interval for the comparison group, and a 24-month observation period is counted back from this average month of participation. The computation is done on a site basis to reduce any inter-site variation that may exist in accepting registrants into the program. A similar calculation is performed to determine the average termination date from WIN at each site. This average termination date at a given site marks the beginning of the "post" observation period for a comparison group member at that site. All comparison group members for a given site are thus assigned the same "pre" and "post" observation periods.

Figure B-3 illustrates the time reference periods for the comparison group.

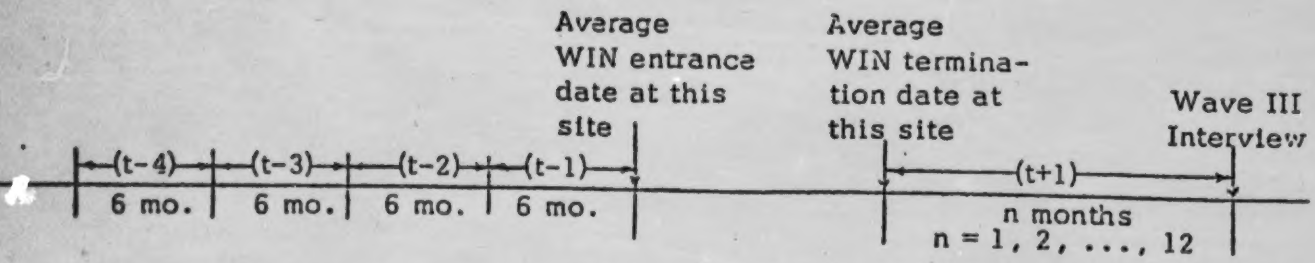
Average
WIN entrance
date at this
site
Average
WIN termina-
tion date at
this site
Wave III
Interview
(t-4)
(t-3)
(t-2)
(t-1)
6 mo.
6 mo.
6 mo.
6 mo.
(t+1)
n months
n = 1, 2, ..., 12

FIGURE B-3

Time Reference for Comparison Group Member at a Given Site

All measurements taken in the "post" period for both groups are annualized to a 12-month, or, more precisely, 48 week, year. This 48 week year arises because the work/welfare calendar in the WIN questionnaire accumulates data on the basis of a four-week month. Over half the WIN participants, and virtually the entire comparison group, have at least nine months of post-WIN observation time. Moreover, since less than ten percent of the participant sample has fewer than five months of observation time, annualization is not a serious problem for the average net impact analysis. To test the sensitivity of our regression model to the variations in the length of the post-WIN observation period, some exploratory regressions were run, weighting the data by the number of observed months in period t+1. These regressions did not produce significantly different results from the unweighted method, and, in view of their greater complexity and cost, were dropped. Since the greater emphasis placed on the larger observation periods by the weighted regressions does not perturb the estimates achieved with the unweighted regression, difficulties arising from annualization do not make the regression estimates of the average net impact problematic.*

B.3 Comparison Group Bias

In this analysis of net average impact of the WIN program, the comparison group assumes the role of a control group in traditional statistical analysis, while the WIN participant group takes on the role of the "treatment group". The two groups should have similar characteristics except for the application of the treatment. Here, of course, the treatment represents actual participation in the WIN program. The effect of the treatment is then determined by comparing the levels of the relevant outcome variables for the two groups.

As discussed in the body of this report (Section III C) the comparison group was initially selected from the WIN registrant pool on the basis of their similarity to sampled participants. Besides being matched on basic demographic characteristics, the comparison group members were eligible for selection into WIN at the time that the treatment group was actually selected for the WIN program. Thus, it would appear at first glance that the comparison group satisfied all of the properties for an experimental control group. However, this contention rests on a shaky foundation, as (1) relatively few characteristics were used in the matching process and (2) some of the unmeasured characteristics might be used as criteria for entry into participant status. Thus, the comparison group may be sufficiently unique in some respect as to bias any comparisons with the participant sample.

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* One cannot conclude from these results that the post-WIN observation period is sufficiently long for all relevant analytical purposes. The length of this period appears to contribute to the problem of measuring the decay of WIN program effects, and causes difficulties in measuring the differential impact of WIN components and services.

In order to guage the extent of potential bias, we examined the pre-WIN employment experiences of both the participant and comparison group samples. Preliminary estimates of the mean values of three outcome variables (earnings, weeks employed and weeks on welfare) indicated that all clients experienced drops in these three outcome variables (viz., a decline in earnings and weeks worked and an increase in weeks on welfare) in the period just prior to the reference time (i.e., the time of WIN entry for participants, average time of WIN entry at each site for the comparison group). Moreover, these drops appeared much larger for participants than for members of the comparison group.* These results suggested that basing the change in outcome variables on the change from a period (e.g., t-1) just prior to the reference time might introduce an upward bias in the value of the dependent variable. To test for the existence and extent of such bias, a multivariate regression model was constructed which included variables X_1 through X_8 discussed in Appendix A and the value of the outcome variabld prior to WIN participation as the dependent variable. Only one outcome variable -- change in earnings--was selected, since all three outcome variables exhibited similar drops. Earnings were measured for the four six-month periods prior to WIN certification -- periods t-1, t-2, t-3, and t-4. For each fixed period, regressions were run estimating the change in earnings for every prior period. This produces six sets of regressions:

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* See Table 3.6 in the main body of the report for actual figures.

(1) change in earnings from period t-4 to t-3;

(2) change in earnings from period t-4 to t-2;

(3) change in earnings from period t-4 to t-1;

(4) change in earnings from period t-3 to t-2;

(5) change in earnings from period t-3 to t-1; and

(6) change in earnings from period t-2 to t-1.

Tables B-1 through B-4 contain the results of these regressions separately for males and females. Regressions (1) through (3) are displayed in Table B-1 (males) and B-3 (females); regressions (4) through (6) are displayed in Table B-2 (males) and Table B-4 (females).

The idea behind these regressions is to test whether or not the two groups experienced significantly dissimilar changes in earnings prior to WIN certification. The key variable to be checked for significance in these regressions is the categorical WIN participant variable. A significant regression coefficient here would indicate that those registrants eventually selected for WIN participation experienced significant changes in earnings relative to those not selected (comparison group), and hence, would present evidence for bias.

Tables B-1 through B-4 uncover this type of bias if period t-1 is used as a base period for males, and if t-1 or t-2 is used as a base period for females. The male participants in the sample have experienced significant drops in earnings in period t-1 relative to the male comparison group. Each regression estimating the change in earnings to period t-1 shows the WIN variable to be negative and statistically significant. Similar results were obtained for the female participants and comparison group members for both periods t-1 and t-2.

These results suggest that use of earnings in the period immediately preceding WIN certification (t-1) to estimate average net impact of WIN on post-period earnings (t+1) will seriously overstate the actual impact WIN had on the participants' average annual earnings rate. Hence, the net impact regressions to estimate WIN's effect on average annual earnings, average annual weeks worked, and average annual weeks on welfare were run with two different base periods. For the males the base periods used were t-1, t-2, and t-2, t-3. For the females, they were t-1, t-2, and t-3, t-4. The regressions using the stable earnings base period are the preferred ones since the two groups were approximately equal during that period. The other set represents a maximum WIN impact due to the temporary imbalance of the two groups just prior to WIN certification.

TABLE B-1

Regressions Estimating Change in Earnings Prior to WIN Certification

Males

	Period (t-3) on (t-4)		Period (t-2) on (t-4)		Period (t-1) on (t-4)	
Variable	Mean (std. dev.)	Reg. Coeff. (t-value)	Mean (std. dev.)	Reg. Coeff. (t-value)	Mean (std. dev.)	Reg. Coeff. (t-value)
26 ≤ Age ≤ 39	.422 (.494)	.333 (2.15)	.422 (.494)	.005 (.023)	.422 (.494)	.039 (.186)
40 ≤ Age	.203 (.402)	-.369 (-1.84)	.203 (.402)	-.437 (-1.55)	.203 (.402)	-.654 (-2.39)
Education 9-11 years	.386 (.487)	-.168 (-.921)	.386 (.487)	-.082 (-.318)	.386 (.487)	-.072 (-.290)
Education ≥ 12 years	.393 (.489)	-.081 (-.450)	.393 (.489)	.324 (1.27)	.393 (.489)	.308 (1.24)
Race (1 = white)	.702 (.458)	.156 (1.09)	.702 (.458)	.497 (2.46)	.702 (.458)	.495 (2.52)
Presence of other wage earner at Wave I	.218 (.413)	-.367 (-2.82)	.218 (.413)	-.083 (-.365)	.218 (.413)	-.086 (-.392)
Presence of child < 6 years at Wave I	.662 (.473)	.079 (.529)	.662 (.473)	.389 (1.84)	.662 (.473)	.211 (1.03)
WIN (1 = participant)	.515 (.500)	-.014 (-.105)	.515 (.500)	-.066 (-.357)	.515 (.500)	-.696 (-3.89)
Earnings in prior period	3.44 (3.97)	-.192 (-11.1)	3.44 (3.97)	-.456 (-18.8)	3.44 (3.97)	-.672 (-28.5)
Change in earnings	-.159 (2.09)		-.292 (3.25)		-1.11 (3.75)	

N = 903
$R^2 = .15$
$\alpha = .458$

N = 903
$R^2 = .31$
$\alpha = .710$

N = 903
$R^2 = .51$
$\alpha = 1.11$

TABLE B-2

Regressions Estimating Change in Earnings Prior to WIN Certification

Males

Variable	Period (t-2) on (t-3) Mean (std. dev.)	Period (t-2) on (t-3) Reg. Coeff. (t-value)	Period (t-1) on (t-3) Mean (std. dev.)	Period (t-1) on (t-3) Reg. Coeff. (t-value)	Period (t-1) on (t-2) Mean (std. dev.)	Period (t-1) on (t-2) Reg. Coeff. (t-value)
26 ≤ Age ≤ 39	.422 (.494)	-.288 (-1.59)	.422 (.494)	-.106 (-.527)	.422 (.494)	.055 (.330)
40 ≤ Age	.203 (.402)	-.251 (-1.09)	.203 (.402)	-.514 (-2.00)	.203 (.402)	-.373 (-1.74)
Education 9-11 years	.386 (.487)	.007 (.034)	.386 (.487)	-.008 (-.033)	.366 (.487)	-.015 (-.073)
Education ≥ 12 years	.393 (.489)	.329 (1.56)	.393 (.489)	.334 (1.43)	.393 (.489)	.136 (.687)
Race (1 = white)	.702 (.458)	.370 (2.20)	.702 (.458)	.429 (2.30)	.702 (.458)	.210 (1.32)
Presence of other wage earner at Wave I	.218 (.413)	.171 (.909)	.218 (.413)	.064 (.304)	.218 (.413)	-.036 (-.202)
Presence of child < 6 years at Wave I	.662 (.473)	.309 (1.76)	.662 (.473)	.175 (.898)	.662 (.473)	-.008 (-.049)
WIN (1 = participant)	.515 (.500)	-.055 (-.359)	.515 (.500)	-.690 (-4.05)	.515 (.500)	-.657 (-4.56)
Earnings in prior period	3.28 (3.78)	-.292 (-13.9)	3.28 (3.78)	-.589 (-25.2)	3.14 (3.53)	-.415 (-19.8)
Change in earnings	-.133 (2.51)		-.954 (3.35)		-.821 (2.58)	

Period (t-2) on (t-3)	Period (t-1) on (t-3)	Period (t-1) on (t-2)
N = 903	N = 903	N = 903
R^2 = .20	R^2 = .45	R^2 = .33
α = .392	α = .923	α = .692

TABLE . B-3

Regressions Estimating Change in Earnings Prior to WIN Certification

Females

	Period (t-3) on (t-4)		Period (t-2) on (t-4)		Period (t-1) on (t-4)	
Variable	Mean (std. dev.)	Reg. Coeff. (t-value)	Mean (std. dev.)	Reg. Coeff. (t-value)	Mean (std. dev.)	Reg. Coeff. (t-value)
26 ≤ Age ≤ 39	.484 (.500)	.088 (1.79)	.484 (.500)	.089 (1.42)	.484 (.500)	.087 (1.38)
40 ≤ Age	.181 (.385)	.071 (1.08)	.181 (.385)	-.044 (-.536)	.181 (.385)	-.105 (-1.26)
Education 9-11 years	.399 (.490)	-.007 (-.114)	.399 (.490)	.045 (.584)	.399 (.490)	.050 (.650)
Education ≥ 12 years	.455 (.498)	.067 (1.11)	.455 (.498)	.127 (1.67)	.455 (.498)	.202 (2.62)
Race (1 = white)	.435 (.496)	.011 (.286)	.435 (.496)	-.098 (-1.96)	.435 (.496)	-.087 (-1.73)
Presence of other wage earner at Wave I	.222 (.416)	-.020 (-.418)	.222 (.416)	.051 (.862)	.222 (.416)	.053 (.884)
Presence of child < 6 years at Wave I	.470 (.499)	-.025 (-.541)	.470 (.499)	-.039 (-.675)	.470 (.499)	-.229 (-3.89)
WIN (1 = participant)	.539 (.499)	-.021 (-.541)	.539 (.499)	-.153 (-3.11)	.539 (.499)	-.313 (-6.29)
Earnings in prior period	.940 (1.78)	-.190 (-17.4)	.940 (1.78)	-.415 (-30.0)	.940 (1.78)	-.638 (-45.6)
Change in earnings	.038 (1.08)		.020 (1.49)		-.119 (1.74)	

Period (t-3) on (t-4)	Period (t-2) on (t-4)	Period (t-1) on (t-4)
N = 2880	N = 2880	N = 2880
$R^2 = .10$	$R^2 = .24$	$R^2 = .43$
$\alpha = .155$	$\alpha = .431$	$\alpha = .648$

TABLE B-4

Regressions Estimating Change in Earnings Prior to WIN Certification

Females

	Period (t-2) on (t-3)		Period (t-1) on (t-3)		Period (t-1) on (t-2)	
Variable	Mean (std. dev.)	Reg. Coeff. (t-value)	Mean (std. dev.)	Reg. Coeff. (t-value)	Mean (std. dev.)	Reg. Coeff. (t-value)
26 ≤ Age ≤ 39	.484 (.500)	.031 (.594)	.484 (.500)	.054 (.892)	.484 (.500)	.040 (.779)
40 ≤ Age	.181 (.385)	-.092 (-1.31)	.181 (.385)	-.134 (-1.68)	.181 (.385)	-.076 (-1.11)
Education 9-11 years	.399 (.490)	.054 (.843)	.399 (.490)	.058 (.789)	.399 (.490)	.030 (.472)
Education ≥ 12 years	.455 (.498)	.089 (1.38)	.455 (.498)	.181 (2.48)	.455 (.498)	.138 (2.17)
Race (1 = white)	.435 (.496)	-.106 (-2.51)	.435 (.496)	-.092 (-1.92)	.435 (.496)	-.030 (-.715)
Presence of other wage earner at Wave I	.222 (.416)	.066 (1.31)	.222 (.416)	.062 (1.09)	.222 (.416)	.024 (.480)
Presence of child < 6 years at Wave I	.470 (.499)	-.024 (-.496)	.470 (.499)	-.221 (-3.95)	.470 (.499)	-.209 (-4.31)
WIN (1 = participant)	.539 (.499)	-.140 (-3.36)	.539 (.499)	-.305 (-6.46)	.539 (.499)	-.224 (-5.47)
Earnings in prior period	.978 (1.78)	-.294 (-25.1)	.978 (1.78)	-.569 (-42.8)	.978 (1.78)	-.409 (-33.5)
Change in earnings	-.018 (1.22)		-.157 (1.61)		-.139 (-1.28)	

	Period (t-2) on (t-3)	Period (t-1) on (t-3)	Period (t-1) on (t-2)
	N = 2880	N = 2880	N = 2880
	R^2 = .19	R^2 = .40	R^2 = .28
	α = .327	α = .586	α = .399

Appendix C: ESTIMATION OF WELFARE GRANT REDUCTION

In this appendix we discuss the additional assumptions underlying the analysis of the effect of WIN participation on welfare grant; these assumptions are required in order to apply the regression models discussed in Appendix A to the separate data base containing longitudinal data on welfare grants. The appendix is organized as follows. Section C.1, Longitudinal Sample for Analysis of Change in Welfare Grant, discusses how the longitudinal welfare data base for this analysis was generated, and also discusses the differences and similarities between the longitudinal sample for the analysis of change in welfare grant and the longitudinal sample for the analysis of other outcome measures, discussed in Appendix B. Section C.2, Bias Introduced by Merging Interview Data with Welfare Record Data, treats some special difficulties which arise in the analysis of pre-WIN observation periods in the longitudinal welfare sample. Lastly, Section C.3, Application of the Regression Model to Analysis of Change in Welfare Grant, discusses the details involved in applying the regression models presented in Appendix A to the longitudinal welfare sample.

C.1 Longitudinal Sample for Analysis of Change in Welfare Grant

The level of AFDC grant is not obtainable from the questionnaire data.* However, during their second-round, management-analysis site visits, the staff of Camil Associates obtained these data through a record search of welfare files. For the present analysis, the most pertinent data collected was recorded as a detailed history of the welfare grant from before WIN registration until the time of the site visit. Each change in welfare grant plus work-related expense and child care was recorded; the amount of child care, but not the amount of work-related expenses, was recorded separately. For purposes of this report, the welfare grant should be taken to include both work-related expenses and child care.

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* This omission is deliberate, as it was thought that serious measurement error would afflict such data.

Although this welfare grant history provides a highly-desirable, additional data source for measuring average net program effects, the analysis of change in welfare grant cannot be conducted without a number of assumptions beyond those required for the three measures of outcome discussed above, in Appendix B. In order to put these assumptions in perspective, it is useful to delineate those assumptions which are common to both the analysis of the change in welfare grant and the analysis of change in weeks employed, in weeks on welfare, and in annual earnings. The following are the principal respects in which the assumptions underlying the two analyses are the same.

- The definition of WIN participation, as discussed in Appendix B, is the same in both analyses.
- The reference time frame, used to define the time periods over which measurements are to be taken, is also the same. That is, periods t, t-1, t-2, t-3, and t-4 refer to exactly the same calendar periods in both analyses. The post-WIN period, t+1, begins at the same time in both analyses.
- The independent variables, except for the lagged variable, grant level in the base period, are the same in both analyses.

The major differences are as follows.

- Since the welfare data are organized around monthly grants, the natural unit for analysis is a monthly, rather than yearly, grant. Moreover, the welfare data are recorded for a normal, calendar month, rather than the four-week month of the work/welfare calendar. For these reasons, the analysis in this appendix proceeds in terms of monthly grants, except where comparison of grant changes with other changes is required. For comparison with other, annualized, measures from the work/welfare calendar, one merely multiplies the monthly estimates by twelve in order to obtain annual estimates of change in welfare grant. Estimates of change in annual grant cited in the main text are obtained in this manner.

- The first observation point on the monthly grant history is the last change in grant level prior to WIN registration, whereas the first observation point on the other change variables from the work/welfare calendar is January 1972. Thus, although the pre-WIN periods, t-1, t-2, t-3, and t-4 refer to the same calendar periods in both analyses, in many cases there will be no welfare data before period t-1 or t-2.
- The last observation point for the monthly grant history is the date of the second round site visit, which need not correspond to the date of the Wave III interview, and occurs generally a few months before the Wave III interview date.

These last two differences may be illustrated by Figure C-1 below.

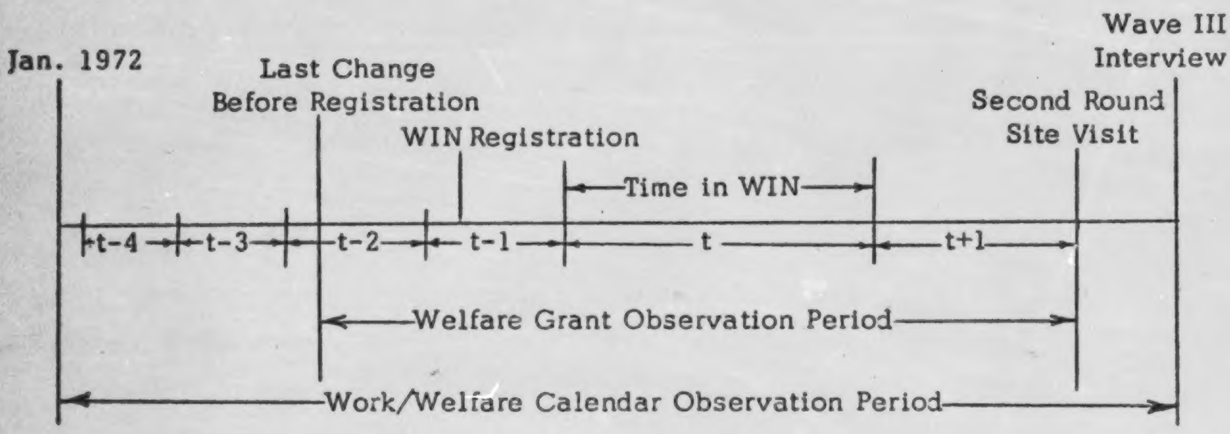

Jan. 1972
Last Change
Before Registration
WIN Registration
Wave III
Interview
Second Round
Site Visit
Time in WIN
t-4
t-3
t-2
t-1
t
t+1
Welfare Grant Observation Period
Work/Welfare Calendar Observation Period

FIGURE C-1

Time Reference for Welfare Grant and Work/Welfare Calendar Observation Periods

In many cases the last entry coded was the last grant change prior to the date of the site visit. After consultation with Camil Associates' staff, this last entry was taken as the grant level from the last date of coding until the date of the site visit in all states where the grant was determined by a flat rate rather than calculated on the basis of fluctuating need. In states which calculate the grant by need, no such extension was made, because such gaps in the data could be merely missing observations, rather than lack of change. However, since grant levels fluctuate much more in states which have calculated grants, it is far less likely that large differences between the last date coded and the time of the site visit will occur in these states.

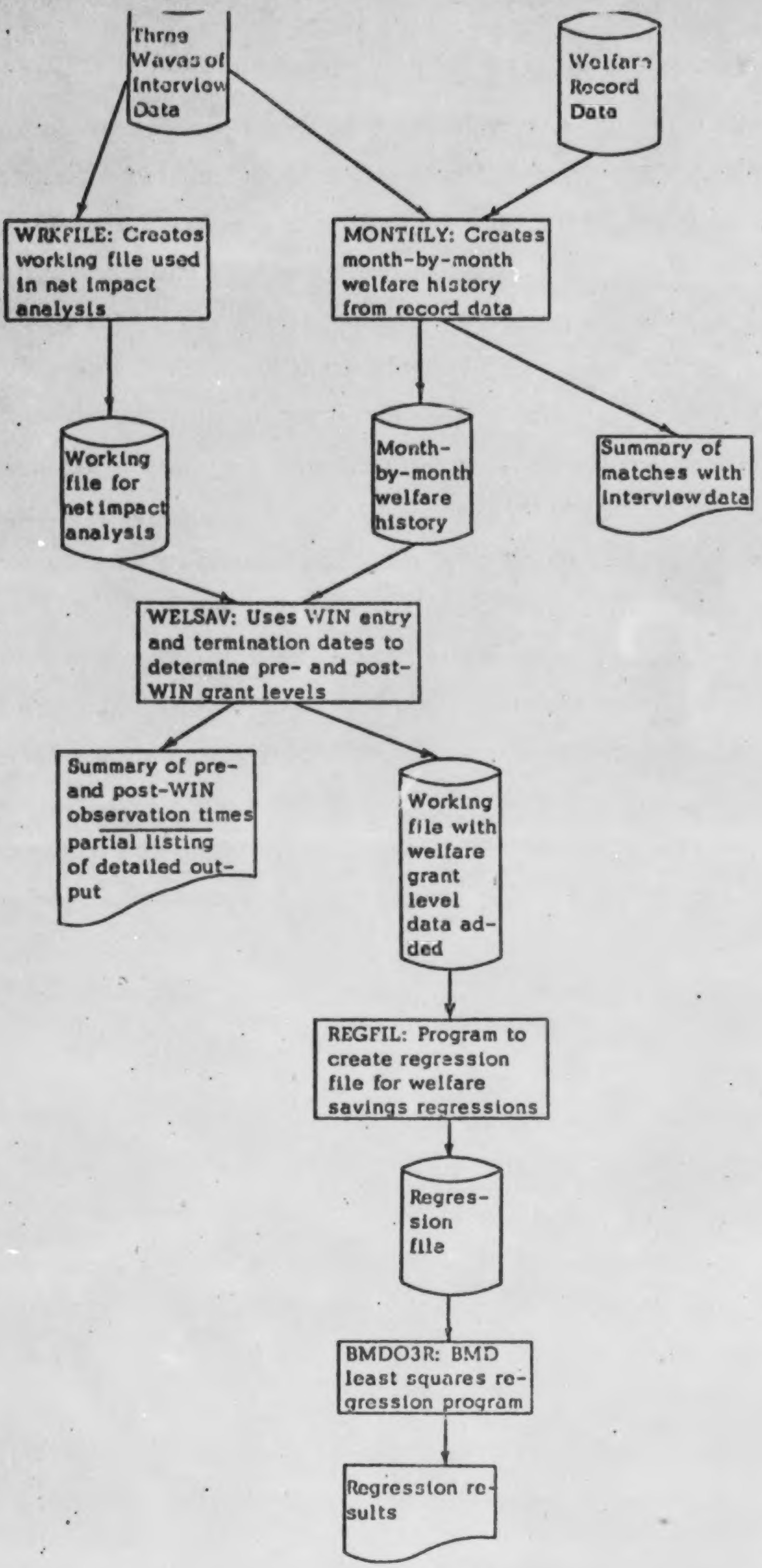
It can be seen from Figure C-1 that the pre- and post-WIN observation periods of welfare grant are shorter than the pre- and post-WIN observation periods on the work/welfare calendar. The extent of this shortening, and its effect on the multivariate analysis of grant change, is discussed in the next section. In the remainder of this section, the procedures for merging the welfare data with the interview data are discussed, and some summary statistics on the number of records merged are presented.

Merging the welfare grant record data with the interview data involved two major steps. First, the coded grant data on changes in welfare grant was converted to a month-to-month history of the welfare grant for the case, similar to the work/welfare history calendar. Second, the data file containing the month-to-month grant history was merged with the interview data, preserving only those grant records which possessed three complete waves of interview data. A merged, "working file" was thereby created, containing whatever welfare grant information was available in periods t+1, t, t-1, t-2, t-3, and t-4, together with the independent variables used in the analysis of change in earnings, in weeks worked, and in weeks on welfare. A flow diagram, depicting the steps involved in preparing the data for analysis, is presented in Figure C-2.

Since this data file contains only those records in the Wave III sample for which a corresponding welfare record could be found, it represents only a subset of the Wave III sample. Overall, 4611, or 87.0 percent, of the 5298 cases in the Wave III sample have a corresponding welfare grant record. The remaining 687 cases in the Wave III sample have no corresponding welfare grant record and were, therefore, excluded from the analysis. Other cases excluded from the analysis fell into three general categories: unknown values for one or more demographic variables; pre- or post-WIN grant unknown (e.g., respondent still in WIN at time of second round site visit); and unknown values for the other outcome variables (change in earnings, weeks employed, and weeks on welfare). The reason for excluding cases with unknown dependent or independent variables is clear: there is no valid way to impute these values. Cases where other outcome variables are unknown were excluded in order to insure that the set of cases in the analysis of welfare grant change is at least a subset of the cases in the analyses of the other outcome variables. Table C-1 summarizes the cases included and excluded from the analysis.

C.2 Bias Introduced by Merging Interview Data with Welfare Record Data

The 561 males and 2165 females included in the analysis all had at least one month of observation in the period t-1. The average monthly grant in each period was computed as the average of all observations that period. Thus, if the data for a case began eight months before WIN entry and ended seven months after WIN termination, an average monthly grant would be computed for t-2, t-1, t, and t+1, but no grant could be computed for periods t-4 or t-3. In this way, there arises a truncation of the sample as the time before WIN entry increases, so that the number of cases with observations in period t-1 is greater than the number of observations in period t-2, the number in


Three Waves of Interview Data
Welfare Record Data
WRKFILE: Creates working file used in net impact analysis
MONTHLY: Creates month-by-month welfare history from record data
Working file for net impact analysis
Month-by-month welfare history
Summary of matches with interview data
WELSAV: Uses WIN entry and termination dates to determine pre- and post-WIN grant levels
Summary of pre- and post-WIN observation times
partial listing of detailed output
Working file with welfare grant level data added
REGFIL: Program to create regression file for welfare savings regressions
Regression file
BMDO3R: BMD least squares regression program
Regression results

Flow Diagram for Regression Analysis of Welfare Savings, Using Three Waves of Interview Data and Record Data on Welfare History

TABLE C-1

Classification of WIN Participant and Comparison Group Samples Used in Analysis of Change in Monthly Welfare Grant

Respondents included in analysis:	Male	Female
WIN Participants	276	1146
Comparison Group	285	1019
TOTAL INCLUDED	561	2165
Respondents excluded due to:		
(1) No match with welfare record data	220	467
(2) Unknown demographic variables (race, age, education, presence of children under six)	72	88
(3) Pre- or post-WIN grant unknown	420	1183
(4) Other outcome variables unknown (change in weeks on welfare, weeks worked, earnings)	35	87
TOTAL EXCLUDED	747	1825

t-2 is greater than in t-3, and the number in t-3 is greater than in t-4. Tables C-2 and C-3 show the average monthly grants and sample sizes for the samples thus defined, for males and females, respectively.

The averages are computed in all periods where observations were available for each of four samples: the full sample of 561 males and 2165 females; the subsample of this full sample which contained observation in period t-2; and the subsamples with observations in periods t-3 and t-4. It can be seen from these tables that the sample size decreases severely for periods t-2, t-3, and t-4, for both males and females. Moreover, the grant levels change as the sample is truncated, suggesting that the truncation is introducing the bias. This effect appears especially pronounced for males. Consider, as an example, the average grant in t+1 for male participants. In the full sample this average is 181.75; in the subsample of cases with observations in period t-2, this average is 216.95. One could perhaps explain this change, by noting that cases with no observations in period t-2 are likely to be transitory welfare cases with lower monthly grants. Removing such cases from the sample would leave cases with higher grants than the average of the full sample. The issue here, however, is not how one might explain the differences between the full sample and the truncated subsamples, but whether this truncation precludes regression analysis of any changes in grant, other than the changes from t-1. In order to investigate this problem further, and to provide some rough estimates of the changes in welfare grant, crude estimates of the changes in grant were constructed for each of the subsamples. As discussed above, in section C.2, with observations of

TABLE C-2

Average Monthly Welfare Grant by Measurement Period

MALES

Period	WIN Participants	Comparison Group
Cases with Observations in Period (t-4)		
(t-4)	211.36	177.89
(t-3)	214.28	183.55
(t-2)	220.20	174.84
(t-1)	232.24	175.97
(t)	237.46	189.11
(t+1)	227.96	154.84
Sample Size	25	38
Cases with Observations in Period (t-3)		
(t-3)	222.33	227.98
(t-2)	241.91	216.83
(t-1)	246.39	205.62
(t)	254.14	202.98
(t+1)	232.54	186.10
Sample Size	57	87
Cases with Observations in Period (t-2)		
(t-2)	242.24	221.66
(t-1)	255.17	213.60
(t)	263.22	204.02
(t+1)	216.95	193.59
Sample Size	115	146
Cases with Observations in Period (t-1)		
(t-1)	240.20	224.70
(t)	235.70	208.41
(t+1)	181.75	190.11
Sample Size	276	285

TABLE C-3

Average Monthly Welfare Grant by Measurement Period

FEMALES

Period	WIN Participants	Comparison Group
Cases with Observations in Period (t-4)		
(t-4)	171.19	153.52
(t-3)	168.34	149.69
(t-2)	172.55	147.57
(t-1)	175.45	150.40
(t)	186.49	155.62
(t+1)	169.27	144.31
Sample Size	171	207
Cases with Observations in Period (t-3)		
(t-3)	167.39	162.14
(t-2)	170.51	169.93
(t-1)	175.55	165.89
(t)	185.50	165.60
(t+1)	166.38	160.46
Sample Size	331	460
Cases with Observations in Period (t-2)		
(t-2)	172.66	165.63
(t-1)	179.04	167.20
(t)	187.34	166.37
(t+1)	169.42	161.92
Sample Size	591	679
Cases with Observations in Period (t-1)		
(t-1)	183.83	175.45
(t)	189.59	172.73
(t+1)	166.86	168.09
Sample Size	1146	1019

change in welfare grant from periods t-4, t-3, and t-2 form subsamples of the full sample for analysis, so that different estimates of change in welfare grant are obtained using each of the subsamples. To investigate the truncation of the sample, crude estimates of the effect of WIN treatment were prepared for each of the four samples. Estimates of the effect of WIN treatment on change in welfare grant from periods t-4, t-3, t-2, and t-1 to period t+1 were generated for the subsample with observations in period t-4, on change in welfare grant from periods t-3, t-2, t-1 to t+1 for the subsample with observations in period t-3, and so on. The results are presented in Tables C-4 and C-5, for males and females, respectively.

One way to address the question of the effect of the truncation is to examine the estimate of the change in grant from period t-1 to t+1 for the full sample and each of the subsamples. Even a cursory look at the tables reveals that the effect of this truncation is severe. Consider first the results for females. Using the full sample, one would estimate a reduction of $9.61 per month from t-1 to t+1 attributable to WIN participation, and this estimate appears significant at the 99 percent confidence level. Using the t-2 sample one would estimate a reduction of only $4.34 per month from t-1 to t+1, using the t-3 sample, a reduction of $3.75 per month and using the t-1 sample, a reduction of $0.64 per month. None of these three results differs significantly from zero, even if one adopts a liberal confidence level of 90 percent. The results for males exhibit the same trend: WIN effects a monthly grant reduction of $23.85 if the full sample is used, and this reduction differs significantly from zero at the 95 percent confidence level. The estimated effects on the monthly welfare grant for the t-2, t-3 and t-4 subsamples are, respectively: a reduction of $18.21, an increase of $5.68, and an increase of $16.85. None of these results are significantly different from zero, and the sample sizes are extremely small, especially in t-3 (sample size = 144) and t-4 (sample size = 53).

TABLE C-4

Crude Estimates of Change in Monthly Welfare Grant

MALES

Base Period	Difference in Means	Standard Error	T-value
Cases with Observations in Period (t-4) (N=53)			
(t-4)	39.65	29.65	1.34
(t-3)	42.39	26.69	1.59
(t-2)	27.76	25.64	1.08
(t-1)	16.85	21.91	.77
Cases with Observations in Period (t-3) (N=144)			
(t-3)	52.08	22.73	2.29
(t-2)	21.36	20.24	1.05
(t-1)	5.68	16.91	.34
Cases with Observations in Period (t-2) (N=261)			
(t-2)	2.77	15.12	.18
(t-1)	-18.21	13.48	-1.35
Cases with Observations in Period (t-1) (N=561)			
(t-1)	-23.85	10.51	-2.27

TABLE C-5

Crude Estimates of Change in Monthly Welfare Grant

FEMALES

Base Period	Difference in Means	Standard Error	T-value
Cases with Observations in Period (t-4) (N=378)			
(t-4)	7.28	10.18	.72
(t-3)	6.65	9.68	.69
(t-2)	.47	9.23	.04
(t-1)	- .64	7.93	- .08
Cases with Observations in Period (t-3) (N=791)			
(t-3)	.66	6.54	.10
(t-2)	- .67	6.04	- .11
(t-1)	-3.75	5.15	- .73
Cases with Observations in Period (t-2) (N=1270)			
(t-2)	- .47	4.79	- .10
(t-1)	-4.34	4.08	-1.06
Cases with Observations in Period (t-1) (N=2165)			
(t-1)	-9.61	3.48	-2.76

These results all point to one conclusion: the truncation introduces a bias into estimates of program effect. It appears, therefore, that any estimates of change in welfare grant which can be constructed are restricted to estimates of changes from t-1 to t+1. Although we cannot be certain that even the changes from period t-1 are completely comparable to the estimates of the other outcome variables, it is encouraging to note that the crude estimates of change in grant from t-1 to t+1 are consistent with crude estimates of change in annual earnings These estimates were $850 increase in annual earnings for males, and a $624 increase for females. The crude estimates given here represent an annual grant reduction of $386 for males, and $115 for femal These modest reductions are consistent with the crude estimate of change in earnings, coupled with the crude estimates of no change in weeks o welfare. It is likely that much of the $624 increase in earnings for females was offset by increases in work related expnese and child care, as well as by application of the income disregard, thus resulting in an annual grant reduction of only $115. A similar effect is observed for males, except the ratio of grant reduction to earnings increase is higher (18 percent for females, 45 percent for males) probably because the income disregard is not applied to males.

C.3 Application of Regression Model to Analysis of Change in Welfare Grant

The regression analysis of change in welfare grant was designed to parallel the analysis of the other three measures of program effect. For the reasons discussed in section C.2, the analysis of change in grant was restricted to changes from t-1 to t+1. These results, therefore, may be regarded as upper bounds on the grant reduction attributable to WIN, since estimates of the reduction using the change from t-2 or t-3 to t+1 would surely be lower. In regressions on the truncated subsample for period t-2 the WIN participation variable was not significantly different from zero. These results are not presented here, since they suffer from the truncation bias discussed above. One could postulate a lower bound of zero for the program effect on welfare grant. However, the true lower bound, using t-2 or t-3 as a base period, may be higher, because the bias introduced by the truncation of the sample tends to deflate the estimated grant reduction one would attribute to WIN participation if the truncated subsample for period t-2 or t-3 is used in the regression analysis.

Estimates of the change in welfare grant from t-1 to t+1 were derived using both models discussed in Appendix A; that is, with a model which introduced no controls for site variation, and with a model which had such controls. It is particularly important to control for site effects in the analysis of change in welfare grant, since grant level is subject to state variation and, hence, site variation. Moreover, it is likely that the truncation bias is correlated with site, since site selection practices affect the length of time from WIN registration to WIN participation, and state payment practices affect the length of time welfare grants tend to remain at constant levels. To the extent that truncation is correlated with site rather than with other independent variables, the effect of the truncation bias on estimates of WIN participation will be controlled for when the model with site controls is used, but not when the model without site controls is used.

The independent variables used to estimate the change in welfare grant were X_1 through X_9 discussed in Appendix A, with X_9 being the dependent variable -- grant level -- in period t-1. Variable, X_{10}, the value of the dependent variable in t-2, was not used, since its use would have restricted the size of the sample and introduced the bias discussed above. In order to estimate the model with site controls, it was necessary to perform a separate computation of the mean values of these independent variables at each site, since the set of observations of grant change is only a subset of the set of observations of the other outcome variables. The detailed results of the regressions, both with, and without site controls, are presented in Appendix D. A summary of the regression estimates of the effect of WIN participation on welfare grant is presented below. The crude estimates are included for the purpose of comparison.

TABLE C-6

Estimates of the Effect of WIN on Monthly Welfare Grant*

Method of Estimation	Males	Females
Crude	-23.85 (-2.27)	-9.61 (-2.76)
Multivariate regression - no site controls	-18.37 (-1.81)	-8.65 (-2.53)
Multivariate regression - controlling for site	-13.70 (-1.15)	-8.90 (-2.53)

* T-values are included in parentheses.

These results show that the regression estimates for females are essentially identical, and predict a reduction in grant only slightly lower than the crude estimates. All three results are sufficiently precise to be declared greater than zero at a confidence level of 95 percent. The regression results for males, however, differ substantially, both from each other and from the crude estimates. Regression, even without site controls, reduces the estimated contribution of WIN participation over the crude estimate, and regression using site controls reduces this estimate even further. The precision of these estimates is such that only the crude estimate would be judged greater than zero at a confidence level of 95 percent.

Taking the estimates using regression with site controls as the best available estimate of the change in grant from period t-1, we may state the following conclusion. The best available estimates of the impact of the WIN program on welfare grants is that the program effects a reduction in grant, both for males and females, from the period just before WIN participation to the period just after WIN termination. The estimate of reduction in grant for females is sufficiently precise to be judged greater than zero by standard statistical tests, but the estimate of reduction in grant for males is not sufficiently precise to be judged greater than zero by standard statistical tests.

Appendix D: DETAILED REGRESSION RESULTS

This appendix contains complete regression results for the analysis of WIN II impact. As discussed in Appendix A, we specified three dependent and thirty-eight independent variables for our analysis, and employed them in a variety of combinations. In particular, we utilized eleven different combinations for each dependent variable, for each sex, for a total of 66 regressions. We performed these calculations for two different base years, thus giving us a total of 132 regressions from the basic model and its extensions. The detailed results for all of these regressions are reported here.

Page 254 contains another descriptive listing of the independent variables used. For convenient reading of the tables that (double-) page should be opened first. In each of the tables we have displayed only those regression coefficients that achieved at least a .10 level of statistical significance, together with their standard deviations. Dashes (-) indicate that a variable was included in a particular regression but failed to attain statistical significance. An empty cell indicates that a variable was not included in that particular specification of the basic model. The eleven specifications utilized are displayed in columns A thru K.

D.1a Change in Earnings: Males
base period: t-1

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	521/2.0	523/2.0	505/2.0	506/2.0	511/2.0	639/2.5	639/2.5	617/2.4	621/2.4	610/2.4	524/2.0
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	642/2.1	542/1.8	545/1.7	546/1.7	—	627/2.1	542/1.8	524/1.7	522/1.7	—	515/1.6
5	629/2.6	508/2.0	472/1.8	476/1.8	478/1.8	533/2.2	434/1.8	—	—	—	475/1.8
6	—	—	—	—	—	—	—	—	—	—	—
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		-664/2.2	—	—	—		-561/1.9	—	—	—	—
10	—	—	—	—	—	-1167/1.7	—	—	—	—	
11						—	—	—	—	—	
12						—	—	—	—	—	
13						—	—	—	—	973/1.7	
14						1989/5.4	1975/5.4	2077/5.6	2088/5.6	2414/6.0	
15			—	—	—			—	—	—	—
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			-874/1.8	-867/1.7	—			—	—	—	—
23			—	—	-679/1.7			—	—	-661/1.7	-691/1.7
24			—	—	-1336/1.7			—	—	—	—
25			—	—	—			—	—	—	—
26			—	—	—			—	—	—	—
27			-1222/3.3	-1212/3.1	-1210/3.1			-1203/3.3	-1132/3.0	-1127/2.9	-1206/3.1
28			—	—	—			—	—	—	—
29			—	—	—			—	—	—	—
30											852/2.1
31					-830/1.9					-859/2.0	—
32					—					—	—
33					-1006/2.5					—	—
34				—	—				—	—	—
35	-778/15.4	-781/15.5	-780/15.3	-780/15.3	-780/15.3	-767/15.4	-770/15.4	-770/15.4	-771/15.4	-772/15.4	-779/15.3
36	254/5.8	262/6.0	253/5.8	253/5.8	253/5.8	245/5.7	252/5.9	246/5.7	246/5.7	247/5.7	253/5.8
37											
38	418/1.9	445/2.0	460/2.0	460/2.0	857/2.1	—					
R^2	.238	.242	.259	.259	265	.264	.267	.284	.284	.288	.264
N	976	976	976	976	976	976	976	976	976	976	976

D.1b Change in Earnings: Males
base period: t-2

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	500/1.9	503/1.9	474/1.8	475/1.8	489/1.8	617/2.3	617/2.3	584/2.2	587/2.2	581/2.2	502/1.9
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	716/2.3	608/1.9	609/1.9	609/1.9	595/1.9	702/2.3	607/2.0	585/1.8	581/1.8	581/1.8	597/1.9
5	668/2.7	542/2.1	517/1.9	520/1.9	514/1.9	581/2.4	477/1.9	—	452/1.7	472/1.8	510/1.9
6	—	—	—	—	—	—	—	—	—	—	—
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		-701/2.3	-636/1.8	-634/1.7	—		-593/1.9	—	—	—	—
10	-1297/1.8	-1259/1.8	—	—	—	-1316/1.9	-1280/1.8	-1184/1.7	—	—	
11						—	—	—	—	—	
12						-1164/1.6	—	—	—	—	
13						—	—	—	—	—	
14						1833/4.9	1823/4.9	1941/5.1	1951/5.1	2287/5.5	
15			—	—	—			—	—	—	—
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			-837/1.7	—	—			—	—	—	—
23			—	—	-691/1.7			—	—	-693/1.7	-705/1.7
24			—	—	-1358/1.7			—	—	—	—
25			—	—	—			—	—	—	—
26			—	—	—			—	—	—	—
27			-1205/3.2	-1198/3.0	-1191/3.0			-1170/3.1	-1100/2.8	-1092/2.8	-1188/3.0
28			—	—	—			—	—	—	—
29			—	—	—			—	—	—	—
30											756/2.7
31					-955/2.1					-989/2.2	—
32					—					—	—
33					-1058/2.5					—	—
34				—	—				—	—	—
35	-636/13.1	-626/12.9	-640/13.0	-641/12.9	-638/12.9	-641/12.4	-633/13.2	-646/13.2	-647/13.2	-646/13.2	-638/12.9
36	—	—	—	—	—	—	—	—	—	—	—
37											
38	—	—	—	—	764/2.7					—	
R2	.279	.284	.299	.299	.305	.302	.305	.320	.321	.325	.304
N	956	956	956	956	956	956	956	956	.956	956	956

D.2a Change in Earnings: Females

base period: t-1

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	189/2.1	189/2.1	190/2.1	189/2.1	187/2.1	186/2.1	185/2.1	186/2.1	185/2.0	184/2.0	181/2.0
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	610/5.5	600/5.3	551/4.8	547/4.8	541/4.8	584/5.3	575/5.1	526/4.6	520/4.6	519/4.6	556/4.9
5	335/4.6	327/4.4	310/3.9	321/4.0	331/4.2	317/4.4	319/4.3	302/3.9	316/4.0	325/4.1	329/4.1
6	—	—	—	—	—	—	—	—	—	—	—
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		—	—	—	—		—	—	—	—	—
10	-588/3.3	-581/3.3	-567/3.2	-558/3.1	—	-590/3.4	-586/3.4	-581/3.3	-571/3.2	—	
11						363/4.4	361/4.4	374/4.5	374/4.5	454/4.6	
12						—	—	—	—	330/1.8	
13						623/4.8	622/4.8	604/4.6	602/4.6	644/4.7	
14						1551/10.7	1558/10.7	1570/10.7	1577/10.8	1566/10.4	
15			292/2.1	323/2.3	342/2.5			284/2.1	321/2.3	332/2.4	349/2.5
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			—	—	—			—	—	—	—
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			—	—	—			—	—	—	—
26			—	—	—			—	—	—	—
27			-278/2.5	-254/2.3	-215/1.9			-262/2.4	-234/2.1	-201/1.8	-217/2.0
28			—	—	—			—	—	—	—
29			-261/1.8	-267/1.9	-238/1.7			-248/1.7	-254/1.8	-235/1.7	—
30											660/7.5
31					—					—	814/6.7
32					-512/2.1					-491/2.0	—
33					-557/4.2					-375/2.8	—
34				—	-696/1.7				-698/1.7	-783/1.9	-759/1.8
35	-549/17.1	-550/17.1	-552/17.1	-551/17.1	-557/17.3	-557/17.5	-558/17.5	-560/17.5	-560/17.5	-563/17.7	-561/17.4
36	189/6.8	189/6.8	188/6.7	188/6.7	191/6.9	192/7.0	192/7.0	191/6.9	190/6.9	193/7.0	193/6.9
37											
38	553/7.5	554/7.5	564/7.6	564/7.6	662/7.5						
R2	.146	.146	.152	.153	.161	.165	.165	.171	.172	.176	.157
N	3026	3026	3026	3026	3026	3026	3026	3026	3026	3026	3026

D.2b Change in Earnings: Females
base period: t-3

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	—	—	—	—	—	—	—	—	—	—	—
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	645/5.6	627/5.4	586/4.9	581/4.9	574/4.9	614/5.3	596/5.1	554/4.7	548/4.7	546/4.7	584/4.9
5	305/4.0	293/3.8	288/3.5	300/3.6	307/3.7	282/3.8	281/3.7	275/3.4	290/3.5	298/3.6	306/3.7
6	—	—	—	—	—	—	—	—	—	—	—
7	-170/1.9	-167/1.9	-202/2.2	-203/2.3	-213/2.4	-179/2.0	-179/2.0	-212/2.4	-214/2.4	-219/2.5	-207/2.3
8		—	—	—	—		—	—	—	—	—
9		—	—	—	—		—	—	—	—	—
10	-626/3.4	-614/3.3	-588/3.1	-582/3.1	—	-627/3.4	-618/3.4	-603/3.2	-594/3.2	—	
11						231/2.7	231/2.7	249/2.9	249/2.9	349/3.4	
12						—	—	—	—	—	
13						471/3.5	469/3.5	457/3.4	454/3.3	512/3.6	
14						1476/9.8	1487/9.8	1500/9.9	1507/9.9	1507/9.6	
15			256/1.8	288/2.0	307/2.1			239/1.7	278/2.0	290/2.0	311/2.2
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			—	—	—			—	—	—	—
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			218/2.0	253/2.2	256/2.3			196/1.8	238/2.1	241/2.2	258/2.3
26			—	—	—			—	—	—	—
27			-258/2.3	-236/2.1	-202/1.8			-248/2.2	-221/1.9	-193/1.7	-205/1.8
28			—	—	—			—	—	—	—
29			-321/2.1	-326/2.2	-297/2.0			-304/2.1	-310/2.1	-290/2.0	-294/2.0
30											548/6.0
31					—					—	657/5.2
32					-484/1.9					-460/1.9	—
33					-622/4.5					-430/3.1	-229/2.0
34				—	—				-723/1.7		-735/1.7
35	-752/20.8	-751/20.8	-753/20.8	-753/20.8	-750/20.8	-755/21.1	-755/21.1	-757/21.1	-756/21.1		-749/20.7
36	181/5.0	180/5.0	177/4.9	176/4.9	173/4.8	184/5.2	184/5.1	181/5.0	180/5.0		173/4.8
37											
38	412/5.4	414/5.4	429/5.6	429/5.6	549/6.0						
R2	.241	.242	.247	.248	.255	.259	.260	.265	.266	.269	.253
N	3011	3011	3011	3011	3011	3011	3011	3011	3011	3011	3011

D.3a Change in Weeks Worked: Males
base period: t-1

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	3.1/2.2	3.2/2.2	2.8/2.0	2.8/2.0	3.0/2.1	3.8/2.7	3.8/2.7	3.5/3.4	3.5/2.5	3.5/2.5	3.0/2.1
2	—	—	—	—	—	3.1/1.7	3.1/1.7	3.1/1.7	3.1/1.7	3.1/1.7	—
3	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—	—	—	—	—
5	3.9/2.9	3.2/2.3	3.2/2.2	3.3/2.2	3.2/2.1	3.4/2.5	2.8/2.0	2.8/1.9	3.0/2.0	3.0/2.0	3.2/2.1
6	3.0/2.0	2.9/1.9	—	—	—	2.5/1.7	2.5/1.7	—	—	—	—
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		-3.6/2.1	-3.7/1.9	-3.6/1.8	-3.6/1.8		-2.9/1.7	—	—	—	-3.6/1.8
10	—	—	—	—	—	—	—	—	—	—	
11						—	—	—	—	—	
12						—	—	—	—	—	
13						—	—	—	—	—	
14						13.0/6.3	12.8/6.3	12.9/6.2	12.9/6.2	13.4/6.0	
15			—	—	—			—	—	—	—
16			4.6/2.0	4.5/2.0	4.3/1.9			4.3/2.0	4.3/1.9	4.2/1.9	4.2/1.9
17			—	—	—			—	—	—	—
18			-4.6/2.0	-4.9/2.0	-4.7/1.9			—	—	—	-4.8/1.9
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			-8.1/2.6	-8.3/2.6	-8.2/2.6			-8.2/2.6	-8.5/2.7	-8.7/2.7	-8.2/2.6
22			-6.0/2.2	-5.8/2.1	-5.6/2.0			-5.1/1.9	-4.7/1.7	-4.6/1.7	-5.6/2.0
23			-4.5/2.0	-4.6/2.1	-4.5/2.0			-4.2/1.9	-4.4/2.0	-4.5/2.0	-4.5/2.0
24			—	—	—			—	—	—	—
25			—	—	—			—	—	4.1/1.6	—
26			—	—	—			—	—	—	—
27			-6.6/3.2	-6.3/2.9	-6.3/2.9			-6.6/3.3	-6.0/2.8	-6.0/2.8	-6.3/2.9
28			—	—	—			—	—	—	—
29			—	5.1/1.6	5.4/1.7			—	—	—	5.4/1.7
30											4.4/2.9
31					—					—	—
32					—					—	5.0/1.9
33					-5.2/2.3					—	—
34				—	—				—	—	—
35	-0.8/17.9	-0.8/17.9	-0.8/17.9	-0.8/17.9	-0.8/17.9	-0.8/18.0	-0.8/18.1	-0.8/18.0	-0.8/18.0	-0.8/18.0	-0.8/17.9
36	0.2/4.2	0.2/4.3	0.2/4.1	0.2/4.1	0.2/4.0	0.1/3.9	0.2/4.0	0.1/3.8	0.1/3.8	0.1/3.8	0.2/4.0
37											
38	3.0/2.4	3.1/2.5	3.3/2.6	3.3/2.6	4.5/2.9						
R2	.312	.316	.338	.338	.342	.339	.341	.360	.361	.362	.341
N	976	976	976	976	976	976	976	976	976	976	976

D.3b Change in Weeks Worked: Males
base period: t-2

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	2.8/1.9	2.9/1.9	2.5/1.7	2.5/1.7	2.7/1.8	3.5/2.4	3.5/2.4	3.1/2.1	3.1/2.1	3.1/2.1	2.7/1.8
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	—	—	—	—	—	—	—	—	—	—	—
5	4.3/3.1	3.6/2.5	3.8/2.5	3.9/2.5	3.6/2.4	3.9/2.8	3.3/2.3	3.4/2.3	3.6/2.4	3.5/2.3	3.6/2.4
6	3.0/1.9	2.9/1.9	—	—	—	—	—	—	—	—	—
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		-4.0/2.3	-4.6/2.3	-4.5/2.3	-4.5/2.2		-3.3/1.9	-3.7/1.9	-3.6/1.8	-3.5/1.7	-4.5/2.2
10	—	—	—	—	—	—	—	—	—	—	
11						—	—	—	—	—	
12						—	—	—	—	—	
13						—	—	—	—	—	
14						11.8/5.6	11.7/5.6	11.8/5.5	11.9/5.5	12.3/5.3	
15			—	—	—			—	—	—	—
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			-4.1/1.6	-4.3/1.7	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			-9.7/2.7	-8.9/2.7	-8.8/2.7			-8.9/2.8	-9.2/2.9	-9.3/2.9	-8.8/2.7
22			-5.3/1.9	-5.1/1.8	-4.9/1.7			—	—	—	-4.9/1.7
23			-4.2/1.9	-4.4/1.9	-4.3/1.9			-4.1/1.8	-4.4/1.9	-4.5/1.9	-4.4/1.9
24			—	—	—			—	—	—	—
25			—	—	—			—	4.6/1.8	4.7/1.9	—
26			—	—	—			—	—	—	—
27			-6.2/2.9	-6.0/2.7	-6.0/2.7			-6.2/3.0	-5.6/2.5	-5.7/2.6	-6.0/2.7
28			—	—	—			—	—	—	—
29			5.6/1.8	5.6/1.8	5.9/1.8			—	—	—	5.9/1.8
30											3.8/2.4
31					—					—	—
32					—					—	5.1/1.9
33					-6.0/2.6					—	—
34				—	—				—	—	—
35	-0.7/15.8	-0.7/15.7	-0.7/15.9	-0.7/15.9	-0.7/16.0	-0.7/16.4	-0.7/16.2	-0.7/16.5	-0.7/165	-0.7/165	-0.7/16.0
36	—	—	—	—	—	—	—	—	—	—	—
37											
38	2.2/1.7	2.3/1.8	2.6/2.0	2.6/2.0							
R2	.355	.358	.378	.378	.383	.377	.380	.396	.397	.399	.383
N	956	956	956	956	956	956	956	956	956	956	956

D.4a Change in Annual Weeks Worked: Females
base period: t-1

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	1.5/1.8	1.5/1.8	1.6/1.9	1.6/1.9	1.6/1.8	1.5/1.7	1.5/1.7	1.5/1.8	1.5/1.8	1.5/1.8	1.5/1.8
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	4.4/4.2	4.6/4.3	4.3/4.0	4.3/4.0	4.3/4.0	4.1/3.9	4.3/4.1	4.0/3.8	4.0/3.8	4.0/3.8	4.4/4.1
5	3.1/4.5	3.2/4.6	3.2/4.3	3.1/4.2	3.2/4.3	2.9/4.3	3.1/4.5	3.1/4.2	3.1/4.1	3.1/4.2	3.2/4.3
6	—	—	—	—	—	—	—	—	—	—	—
7	-1.4/1.8	-1.5/1.8	-1.8/2.2	-1.8/2.2	-1.9/2.3	-1.5/1.9	-1.5/1.9	-1.9/2.3	-1.9/2.3	-2.0/2.4	-1.8/2.3
8		—	—	—	—		—	—	—	—	—
9		—	—	—	—		—	—	—	—	—
10	-4.9/3.0	-5.1/3.1	-4.7/2.8	-4.7/2.8	—	-5.0/3.0	-5.1/3.1	-4.8/2.9	-4.8/2.9	—	
11						3.6/4.6	3.5/4.5	3.7/4.7	3.7/4.7	4.0/4.3	
12						—	—	—	—	—	
13						5.5/4.5	5.6/4.6	5.5/4.5	5.5/4.5	5.6/4.4	
14						12.7/9.2	12.7/9.2	12.7/9.3	12.7/9.2	12.5/8.8	
15			2.7/2.1	2.6/2.0	2.7/2.1			2.6/2.1	2.6/2.0	2.6/2.0	2.8/2.1
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			—	—	—			—	—	—	—
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			1.7/1.7	—	—			—	—	—	—
26			—	—	—			—	—	—	—
27			-2.0/1.9	-2.0/2.0	—			-1.8/1.7	-1.8/1.8	—	—
28			—	—	—			—	—	—	—
29			—	—	—			—	—	—	—
30											5.5/6.6
31					—					—	7.3/6.4
32					-4.1/1.8					-4.0/1.8	—
33					-3.8/3.1					-2.4/1.9	—
34				—	—				—	—	—
35	-0.6/22.3	-0.6/22.3	-0.6/22.3	-0.6/22.3	-0.6/22.4	-0.6/22.7	-0.6/22.7	-0.6/22.7	-0.6/22.7	-0.6/22.8	-0.6/22.5
36	0.1/4.4	0.1/4.3	0.1/4.2	0.1/4.2	0.1/4.3	0.1/4.4	0.1/4.3	0.1/4.3	0.1/4.3	0.1/4.3	0.1/4.3
37											
38	4.9/7.1	4.9/7.0	5.0/7.2	5.0/7.2	5.5/6.7						
R2	.230	.231	.238	.238	.242	.242	.243	.249	.249	.252	.239
N	3026	3026	3026	3026	3026	3026	3026	3026	3026	3026	3026

D.4b Change in Weeks Worked: Females
base period: t-3

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	—	—	—	—	—	—	—	—	—	—	—
2	—	—	—	—	—	—	—	—	—	—	—
3	—	—	—	—	—	—	—	—	—	—	—
4	4.6/4.2	4.7/4.3	4.6/4.1	4.6/4.1	4.5/4.1	4.3/4.0	4.4/4.1	4.3/3.9	4.3/3.9	4.3/3.9	4.6/4.2
5	2.8/4.0	2.9/3.9	3.0/3.8	2.9/3.8	3.0/3.9	2.6/3.7	2.8/3.8	2.8/3.7	2.9/3.7	2.9/3.8	3.0/3.8
6	—	—	—	—	—	—	—	—	—	—	—
7	-2.4/2.8	-2.4/2.8	-2.8/3.3	-2.8/3.3	-2.9/3.4	-2.4/2.9	-2.4/3.0	-2.8/3.5	-2.9/3.5	-2.9/3.5	-2.8/3.4
8		—	-2.2/1.9	-2.3/2.0	-2.2/1.9		—	—	—	—	-2.2/1.9
9		—	—	—	—		—	—	—	—	—
10	-4.8/2.8	-4.9/2.8	-4.4/2.5	-4.4/2.5	—	-4.8/2.8	-4.9/2.8	-4.6/2.6	-4.5/2.6	—	
11						2.6/3.2	2.6/3.2	2.7/3.4	2.8/3.4	3.8/3.5	
12						—	—	—	—	—	
13						4.2/3.3	4.2/3.3	4.2/3.3	4.1/3.3	4.5/3.4	
14						12.3/8.7	12.3/8.6	12.3/8.7	12.3/8.6	12.8/8.3	
15			2.6/1.9	2.5/1.9	2.6/1.9			2.4/1.8	2.4/1.8	2.5/1.8	2.7/2.0
16			2.3/1.7	2.1/1.7	2.5/2.0			—	—	2.4/1.9	2.7/2.1
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	2.3/1.7			—	—	2.3/1.8	2.4/1.8
22			—	—	—			—	—	—	—
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			3.5/2.4	3.4/2.3	3.4/2.3			2.3/2.2	2.3/2.2	2.2/2.1	2.4/2.3
26			—	—	—			—	—	—	—
27			—	—	—			—	—	—	—
28			—	—	—			—	—	—	—
29			-3.5/1.8	-3.5/1.8	—			-2.4/1.7	-2.4/1.7	—	—
30											4.7/5.5
31					—					—	6.1/5.1
32					-5.0/3.1					-4.8/2.1	—
33					-4.2/3.3					-2.7/2.1	—
34				—	—				—	—	—
35	-0.8/25.7	-0.8/25.7	-0.9/25.9	-0.8/25.8	-0.8/25.9	-0.8/26.0	-0.8/26.0	-0.8/26.1	-0.8/26.1	-0.8/26.1	-0.8/25.8
36	0.1/4.1	0.1/4.2	0.1/4.1	0.1/4.1	0.1/4.1	0.1/4.2	0.1/4.2	0.1/4.2	0.1/4.2	0.1/4.3	0.1/4.1
37											
38	3.8/5.3	3.8/5.3	4.0/5.5	4.0/5.5	?						
R2	.333	.333	.340	.340	.344	.345	.345	.351	.352	.354	.343
N	3011	3011	3011	3011	3011	3011	3011	3011	3011	3011	3011

D.5a Change in Weeks on Welfare: Males

base period: t-1

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	4.2/2.8	4.2/2.8	4.6/3.0	4.6/3.0	4.5/3.0	3.9/2.6	3.9/2.6	4.3/2.9	4.3/2.8	43/2.8	4.4/2.9
2	6.4/3.3	6.5/3.4	6.7/3.5	6.7/3.5	6.6/3.4	6.1/3.1	6.2/3.2	6.3/3.2	6.3/3.2	6.3/3.2	6.6/3.4
3	—	—	—	—	—	—	—	—	—	—	—
4	-4.1/2.3	-3.6/2.0	-4.1/2.2	-4.1/2.2	-3.9/2.1	-3.9/2.2	-3.5/1.9	-3.9/2.1	-3.9/2.1	-3.8/2.1	-3.9/2.1
5	-3.2/2.2	-2.6/1.8	-2.7/1.7	-2.9/1.9	-2.7/1.7	-2.9/2.0	—	—	-2.8/1.7	-2.6/1.7	-2.7/1.7
6	-6.6/4.1	-6.4/4.0	-6.4/3.9	-6.4/3.9	-6.2/3.8	-6.3/3.9	-6.2/3.8	-6.2/3.8	-6.1/3.7	-6.0/3.7	-6.4/3.9
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		3.3/1.8	—	—	—		—	—	—	—	—
10	8.5/2.6	8.3/2.0	9.1/2.1	8.9/2.1	—	8.5/2.0	8.4/2.0	9.2/2.2	9.0/2.1	—	
11						—	—	—	—	—	
12						9.8/2.3	9.4/2.2	7.5/1.8	7.9/1.9	—	
13						—	—	—	—	—	
14						-53/2.4	-5.2/2.4	-5.4/2.4	-5.6/2.5	-5.7/2.4	
15			4.1/1.7	4.0/1.6	4.0/1.6			4.4/1.8	4.1/1.7	4.1/1.7	4.0/1.6
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	5.6/1.7	5.7/1.7			—	—	—	5.7/1.7
22			—	—	—			—	—	—	—
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			—	—	—			—	—	—	—
26			—	—	—			—	—	—	—
27			5.3/2.4	4.7/2.0	4.8/2.1			5.0/2.3	4.2/1.8	4.3/1.9	4.8/2.1
28			—	—	—			—	—	—	—
29			-6.1/1.9	-6.2/1.9	-6.4/1.9			—	—	—	-6.4/1.9
30											—
31					—					—	—
32					—					—	—
33					4.9/2.0					—	4.2/2.0
34				—	—					—	—
35	-0.8/16.5	-0.8/16.6	-0.8/16.8	-0.8/16.9	-0.8/16.9	-0.8/16.6	-0.8/16.7	-0.8/16.9	-0.8/16.9	-0.8/17.0	-0.8/16.9
36	0.1/1.7	0.1/1.7	—	—	—	0.1/1.7	0.1/1.7	—	—	—	—
37											
38	—	—	—	—	—						
R^2	.346	.349	.369	.370	.374	.355	.357	.376	.377	.379	.372
N	976	976	976	976	976	976	976	976	976	976	976

D5b Change in Weeks on Welfare: Males

base period: t-2

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	4.8/3.1	4.8/3.1	5.2/3.4	5.2/3.4	5.1/3.3	4.5/2.9	4.5/2.9	4.9/3.2	4.9/3.2	4.8/3.1	5.0/3.3
2	8.3/4.2	8.4/4.3	8.5/4.3	8.5/4.3	8.4/4.3	7.9/4.0	8.0/4.1	8.0/4.1	8.0/4.1	8.0/4.1	8.4/4.3
3	—	—	—	—	—	—	—	—	—	—	—
4	-4.3/2.3	-3.8/2.1	-4.2/2.2	-4.2/2.2	-4.1/2.2	-4.1/2.3	-3.7/2.0	-4.1/2.2	-4.1/2.2	-4.0/2.1	-4.1/2.2
5	-3.0/2.0	-2.5/1.6	-2.5/1.6	-2.8/1.7	—	-2.7/1.8	-2.3/1.5	-2.2/1.4	-2.6/1.6	-2.4/1.5	—
6	-7.3/4.4	-7.1/4.3	-7.1/4.3	-7.1/4.3	-6.9/4.2	-7.0/4.3	-6.8/4.2	-6.9/4.1	-6.7/4.1	-6.7/4.0	-7.0/4.2
7	—	—	—	—	—	—	—	—	—	—	—
8		—	—	—	—		—	—	—	—	—
9		—	—	—	—		—	—	—	—	—
10	9.5/2.0	8.3/2.0	9.2/2.1	9.0/2.1	—	8.6/2.0	8.4/2.0	9.3/2.2	9.0/2.1	—	
11						—	—	—	—	—	
12						10.2/2.4	9.9/2.3	8.0/1.9	8.4/2.0	—	
13						—	—	—	—	—	
14						-5.1/2.3	-5.1/2.3	-5.3/2.3	-5.5/2.4	-5.2/2.1	
15			4.4/1.8	4.2/1.7	4.2/1.7			4.6/1.9	4.4/1.8	4.3/1.8	4.2/1.7
16			—	—	—			—	—	—	—
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	5.8/1.7	5.7/1.7			—	5.7/1.7	—	5.7/1.7
22			—	—	—			—	—	—	—
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			—	—	—			—	—	—	—
26			—	—	—			—	-6.0/1.7	—	—
27			5.4/2.4	4.8/2.1	5.0/2.1			5.1/2.3	4.3/1.8	4.5/1.9	5.0/2.1
28			—	—	—			—	—	—	—
29			-6.0/1.8	-6.0/1.8	-6.1/1.8			—	—	—	-6.1/1.8
30											—
31					—					—	—
32					—					—	—
33					4.6/1.9					—	4.3/2.0
34				—	—				—	—	—
35	-0.8/14.3	-0.8/14.3	-0.8/14.4	-0.8/14.5	-0.8/14.5	-0.8/14.3	-0.8/143	-0.8/14.4	-0.8/14.5	-0.8/14.5	-0.8/146
36	—	—	—	—	—	—	—	—	—	—	—
37											
38	—	—	—	—							
R2	.441	.443	.462	.463	.466	.448	.449	.468	.468	.470	.465
N	956	956	956	956	956	956	956	956	956	956	956

D.6a Change in Weeks on Welfare: Females

base period: t-1

Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	2.3/2.8	2.3/2.8	2.4/2.9	2.4/2.9	2.4/2.9	2.4/2.9	2.3/2.8	2.4/3.0	2.5/3.0	2.5/3.0	2.5/3.0
2	2.9/2.6	2.9/2.6	2.8/2.6	2.8/2.5	2.8/2.5	2.9/2.7	2.9/2.6	2.8/2.6	2.8/2.5	2.8/2.5	2.9/2.6
3	1.7/1.7	—	—	—	—	—	—	—	—	—	—
4	-1.7/1.7	-1.7/1.7	-2.4/2.4	-2.4/2.3	-2.3/2.3	—	—	-2.2/2.2	-2.1/2.1	-2.1/2.1	-2.4/2.4
5	-3.7/5.6	-3.4/5.0	-3.7/5.2	-3.9/5.5	-3.9/5.5	-3.6/5.4	-3.3/4.9	-3.7/5.2	-3.9/5.4	-3.9/5.4	-3.9/5.4
6	-4.3/5.5	-4.3/5.5	-4.3/5.5	-4.4/5.5	-4.3/5.5	-4.3/5.5	-4.3/5.5	-4.3/5.5	-4.4/5.6	-4.4/5.5	-4.3/5.5
7	3.7/4.8	3.6/4.7	3.8/4.9	3.8/4.9	3.8/4.9	3.7/4.8	3.6/4.7	3.8/4.9	3.8/4.9	3.9/4.9	3.8/4.9
8		1.6/2.0	—	—	—		—	—	—	—	—
9		2.0/2.4	2.7/2.9	2.0/2.0	2.0/2.0		1.7/2.1	2.5/2.7	1.7/1.7	1.7/1.7	1.9/1.9
10	4.0/2.5	3.9/2.5	4.9/3.0	4.7/2.9	—	4.0/2.5	3.9/2.5	4.9/3.1	4.8/3.0	4.1/2.1	
11						—	—	—	—	—	
12						—	—	—	—	—	
13						—	—	—	—	—	
14						-7.0/5.2	-6.8/5.1	-6.6/5.0	-6.7/5.0	-6.7/4.9	
15			2.1/1.7	—	—			2.2/1.8	—	—	—
16			4.7/4.0	4.3/3.7	4.3/3.6			4.9/4.2	4.5/3.8	4.5/3.7	4.2/3.5
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			1.8/1.7	2.3/2.1	2.4/2.1			—	2.2/2.0	2.2/2.0	2.6/2.3
23			1.9/1.7	2.1/1.9	2.1/1.9			—	2.0/1.8	2.0/1.8	2.2/2.0
24			—	—	—			—	—	—	—
25			—	—	—			—	—	—	—
26			—	—	—			—	-2.2/1.7	-2.2/1.7	—
27			-1.7/1.7	-2.0/2.0	-2.0/2.0			-1.8/1.8	-2.2/2.2	-2.1/2.1	-2.0/2.0
28			—	—	—			—	—	—	—
29			—	—	—			—	—	—	—
30											-1.6/2.0
31					—					—	—
32					—					—	—
33					—					—	—
34				8.7/2.3	8.9/2.4				9.5/2.5	9.5/2.5	9.3/2.5
35	-0.7/23.4	-0.7/23.4	-0.7/23.4	-0.7/23.5	-0.7/23.4	-0.7/23.7	-0.7/23.7	-0.7/23.6	-0.7/23.7	-0.7/23.7	-0.7/23.5
36	—	—	—	—	—	—	—	—	—	—	—
37											
38	-1.4/2.1	-1.4/2.2	-1.2/1.9	-1.2/1.9	-1.6/2.0						
R2	.284	.285	.295	.296	.297	.289	.291	.300	.302	.302	.295
N	3026	3026	3026	3026	3026	3026	3026	3026	3026	3026	3026

D.6b Change in Weeks on Welfare: Female
base period: t-3

V. Variable \ Regression	A	B	C	D	E	F	G	H	I	J	K
1	2.9/3.3	2.7/3.2	2.8/3.3	2.8/3.4	2.8/3.4	2.8/3.3	2.7/3.3	2.8/3.4	2.9/3.4	2.9/3.4	2.9/3.4
2	3.2/2.8	3.1/2.8	3.1/2.8	3.1/2.7	3.1/2.7	3.2/2.9	3.2/2.8	3.2/2.8	3.1/2.8	3.1/2.8	3.2/2.8
3	—	—	—	—	—	—	—	—	—	—	—
4	-1.8/1.8	-1.8/1.7	-2.4/2.3	-2.4/2.3	-2.4/2.2	—	—	-2.2/2.1	-2.1/2.0	-2.1/2.0	-2.4/2.3
5	-3.9/5.6	-3.4/4.9	-3.7/5.1	-4.0/5.4	-4.0/5.4	-3.8/5.5	-3.4/4.9	-3.7/5.0	-3.9/5.3	-3.9/5.3	-4.0/5.3
6	-4.8/5.9	-4.8/6.0	-4.8/5.9	-4.8/5.9	-4.8/5.9	-4.7/5.9	-4.8/5.9	-4.7/5.9	-4.8/6.0	-4.8/5.9	-4.8/5.9
7	4.5/5.7	4.4/5.6	4.6/5.8	4.6/5.8	4.6/5.8	4.5/5.8	4.4/5.6	4.6/5.8	4.6/5.9	4.7/5.9	4.6/5.8
8		1.6/1.9	—	—	—		—	—	—	—	—
9		2.3/2.9	2.8/2.5	1.9/1.8	1.8/1.9		2.0/2.5	2.5/2.7	—	—	1.8/1.7
10	4.1/2.5	3.9/2.3	4.6/2.7	4.4/2.7	—	4.8/2.5	3.9/2.4	4.6/2.8	4.5/2.7	4.1/2.0	
11						—	—	—	—	—	
12						—	—	2.9/1.7	3.0/1.8	2.9/1.7	
13						—	—	—	—	—	
14						-6.7/5.0	-6.6/4.8	-6.3/4.7	-6.5/4.8	-6.5/4.6	
15			2.9/2.2	2.2/1.7	2.1/1.7			2.9/2.3	2.3/1.8	2.3/1.8	2.1/1.6
16			4.7/4.0	4.3/3.5	4.3/3.5			4.9/4.1	4.4/3.6	4.4/3.6	4.2/3.5
17			—	—	—			—	—	—	—
18			—	—	—			—	—	—	—
19			—	—	—			—	—	—	—
20			—	—	—			—	—	—	—
21			—	—	—			—	—	—	—
22			—	2.2/1.9	2.3/2.0			—	2.0/1.8	2.1/1.8	2.4/2.1
23			—	—	—			—	—	—	—
24			—	—	—			—	—	—	—
25			—	—	—			—	—	—	—
26			—	-2.2/1.6	-2.2/1.7			—	-2.4/1.8	-2.4/1.8	-2.2/1.6
27			—	—	—			—	—	—	—
28			—	—	—			—	—	—	—
29			—	—	—			—	—	—	—
30											—
31					—					—	—
32					—					—	—
33					—						—
34				11.1/1.8	11.2/2.9				11.9/3.1	11.9/3.1	11.5/2.9
35	-0.9/24.9	-0.9/25.0	-0.9/24.9	-0.9/24.9	-0.9/24.9	-0.9/24.9	-0.9/24.9	-0.9/24.9	-0.9/24.9	-0.9/24.9	-0.9/24.9
36	—	—	—	—	—	—	—	—	—		—
37											
38	—	—	—	—	—						
R2	.524	.525	.531	.532	.533	.528	.529	.535	.531	.537	.532
N	3011	3011	3011	3011	3011	3011	3011	3011	3011	3011	3011

TABLE D-7

Change in Monthly Welfare Grant Plus Work Related Expenses from Period (t-1) to (t+1)

Males

Variable	Mean	Standard Deviation	Site controlled estimates		Estimates without site control	
			Regression Coefficient	T-value	Regression Coefficient	T-value
26 ≤ Age ≤ 39	.401	.491	-5.55	-.39	3.48	.29
40 ≤ Age	.233	.423	-28.04	-1.87	-10.72	-.72
Education 9 - 11 years	.389	.488	-27.94	-1.57	-10.01	-.71
Education ≥ 12 years	.385	.487	-38.49	-2.20	-31.15	-2.22
Race (1 = white)	.724	.448	-12.79	-.76	-7.80	-.69
Presence of other wage earner at Wave I	.205	.404	-17.49	-1.13	-18.29	-1.42
Presence of child < 6 at Wave I	.645	.479	-22.16	-1.61	-13.61	-1.17
WIN participation (1 = participant)	.492	.500	-13.70	-1.15	-18.37	-1.81
Grant in period (t-1)	232.32	118.13	-.36	-6.82	-.33	-7.68
Change in grant from period (t-1) to (t+1)	-46.33	125.10				

N = 561

$R^2 = .218$

$R^2 = .119$

$\alpha = 75.307$

Table D.8

Change in Monthly Welfare Grant Plus Work Related Expenses from Period (t-1) to (t+1)

Females

Variable	Mean	Standard Deviation	Site controlled estimates		Estimates without site control	
			Regression Coefficient	T-value	Regression Coefficient	T-value
26 ≤ Age ≤ 39	.479	.500	9.21	2.08	7.10	1.62
40 ≤ Age	.181	.385	8.42	1.42	5.00	.85
Education 9 - 11 years	.397	.489	9.18	1.62	9.72	1.80
Education ≥ 12 years	.461	.499	-5.75	-.98	-2.07	-.39
Race (1 = white)	.424	.494	-12.28	-2.53	-9.33	-2.66
Presence of other wage earner at Wave I	.228	.419	-24.09	-5.77	-24.07	-5.88
Presence of child < 6 at Wave I	.492	.500	16.38	3.91	13.62	3.36
WIN participation (1 = participant)	.529	.499	-8.90	-2.53	-8.65	-2.53
Grant in period (t-1)	179.88	102.20	-.23	-10.68	-.15	-8.73
Change in grant from period (t-1) to (t+1)	-12.44	81.07				

N = 2165

R^2 = .156

R^2 = .069

α = 14.297

Age 26-39

Age ≥ 40

Education 9-11

Education ≥ 12

Race (1=White)

Presence of other wage earner at Wave I

Presence of Child under 6 at Wave I

Moderate unemployment

High unemployment

WIN Service level 1 (unassigned participants)

WIN Service level 2 (low level of services: other than levels 3-5)

WIN Service level 3 (education: 06,07,08, or 22)

WIN Service level 4 (training: 04 or 05)

WIN Service level 5 (OJT/PSE: 11, 12, or 13)

ES controls appraisal services & manpower (0303/3) and Employability is used at appraisal to determine participation (0309.1/1)

Unknown how aopraisal control is exercised (0308/8 or 9)

Site maintains a formal unassigned participant pool used (0401/1)

Unknown whether unassigned participant pool used (0401/9)

Stop status is of such a duration or frequency that it impedes progress of the client (0601/1)

Unknown whether stop status impedes progress of the client (0601/9)

Counseling is extensively used, (1001/4)

Institutional training more than minor part or good choice of skills (1101/2 or 3)

Institutional training, limited availability (1103/1 or 2)

Institutional training, N.A. (1103/8)

Little or no retroactive placements (1914/0 or 1)

Unknown whether retroactive placements used (1914/8 or 9)

Little or no manpower resources available (2005/0 or 1)

Little or not OJT and PSE (1401/0 and 1501/3)

OJT and PSE considered as major option (1401/3 and 1501/3)

Job Search level 1 (not used)

Job Search level 2 (informal)

Job Search level 3 (formal)

Job Search level 4 (other)

Site response rate

Dependent variable in prior period*

Dependent variable in period before prior period**

Dependent variable in regression***

WIN Service level 2 through 5

---

* For males this is annualized on the basis of (t-2), for females (t-3).

** For males this is annualized on the basis of (t-3), for females (t-4).

*** Annualized change in dependent variable, i.e., (post-WIN)-(pre-WIN), based on (t+1) for females, and (t-3) for females

Appendix E. EXEMPTEE EXPERIENCES

A minor task of the study effort was to determine what happened to AFDC recipients who were exempted from WIN participation requirements. As discussed in Section IIC. 1, AFDC recipients were exempted from WIN participation for a variety of reasons, most commonly childcare responsibilities or poor health. The purpose of this appendix is to describe how the welfare status of exemptees changed during the study period (1974-75).

E.1 Changes in Welfare Status, 1974-75:

Changes in the welfare status of exemptees were determined by review of 750 exemptee records. Unlike the rigorous methods employed for the longitudinal sample of registrants and participants, the method used to select the sample of exemptees was quite simple: at each site, from 10 to 20 recent exemptees (usually those reviewed the same day as the visit) were selected and followed up one year later. Since no attempt was made to determine the total number of exemptees reviewed during the same period, the results were not weighted and do not necessarily represent the precise percentage of events occurring nationally. However, within each site, this selection was random and unbiased, and the results, taken as a whole, are generally reflective of what does happen to the exemptee population.

Overall, about one third of all persons exempted just prior to the first site visit in 1974 were off welfare in 1975. For males, the total was just under 50 percent, for females (representing nearly 90 percent of all exemptees;

Table E.1 1975 Welfare Status of 1974 Exemptees, by Sex

	MALE		FEMALE	
	Still on Welfare	Off Welfare	Still on Welfare	Off Welfare
Employment	29%		22%	
Left area	--		26%	
Failure to report for Win redetermination	11%		11%	
Transfer to other welfare category	14%		4%	
Other, including marriage, change in support level	32%		31%	
Unknown	14%		7%	

see section IIC. 1), about 32 percent were off welfare by 1975.* As also shown in Table E.1, the principal reason for leaving welfare was a combination of factors including remarriage, change in eligibility status, increase in support (34 percent), followed by employment (25 percent), and left area (just under 25 percent). As would be expected, the principal reason for males was about equally divided between employment (24%) and a combination of other reasons (about 30%). For women the combination of other reasons (3 percent) and left area (26 percent) were the principal reasons, followed by employment (22 percent).**

Although these figures are tentative, they are interesting in that they demonstrate the highly dynamic state of the welfare population, even for those persons exempted from the work test and WIN services. These findings also conform to those found from other studies,*** and it is reasonable to conclude, therefore, that both the high discontinuance rates, and the reasons for discontinuance are generally representative of the AFDC exemptee population.*

---

* A number of women who were still on welfare had some employment, or were currently employed at the time of the 1975 coding, but this was not separately analyzed.

** Again, a number of women were still receiving welfare, but working.

*** Bradley Schiller "Turnover Rates in Public Assistance Programs," Welfare in Review, September 1970; KETRON, Inc., "Multivariate Regression Analysis of Annual Natural Turnover Rate using 1969 and 1971 AFDC National Surveys," August, 1973; and "multivariate Regression Analysis of Annual AFDC Discontinuance Rates and AFDC dependency," November 11, 1974; and Camil Associates, Inc., "Welfare Dependency, Termination, and Employment," 1972.

Of those 1974 exemptees still on welfare in 1975, 85 percent were still exempt as of 1975. Moreover, there was little change in the basis for exemption between the two years, as shown below:

Reason for Exemption in 1974	Percentage of Category Still on Welfare Still Exempt in 1975
Under 16	80 percent
Attending school	45 percent
Ill or incapacitated	78 percent
Remoteness	92 percent
Family illness	86 percent
Children under six	87 percent
Care at home	60 percent
Combination of reasons	100 percent

Whether the lack of change reflects actual continuation of the reason for exemption, or a failure to review the exemption status could not be determined. However, of those persons no longer exempt in 1975, 50 percent had entered WIN, a higher percentage than for the registrant population as a whole.

Appendix F. SITE CHARACTERISTICS AND IMPACT

Our analysis of individual service receipt (Chapter VI) suggests that variations in local WIN program structure will have a marked influence on the success rate of local projects. In particular, it appears highly likely that "full component" sites -- those offering all employment services -- will tend to have the highest performance ratings, as the level of individual service is directly related to net impact. In order to derive greater insight into the effectiveness of program structure, we attempted to disaggregate site structures into their many features, then examine the relationship of each feature to participant outcomes. Our analysis was severely constrained, however, by lack of resources in the final stages of analysis and must be regarded as incomplete. Neither a quality rating of local sites and components nor a formal factor analysis of component/outcome relationships was undertaken. Nevertheless, a brief review of our preliminary explorations may be of interest, and are described here.

F. 1 Site Characteristics

In order to assess the potential importance of site characteristics on WIN performance, we examined the two-way (not multivariate) relationship between observed changes in participant earnings and the following characteristics of local WIN sites:

- ° The degree to which registration is a problem.
- ° Whether the site used pre-appraisal.

- How ES and SAU staff participate in appraisal.
- How ES and SAU staff control the appraisal process.
- The degree to which the applicant's employability potential is considered in appraisal.
- The degree to which demographic characteristics are considered in appraisal.
- Whether language ability is considered in appraisal.
- Whether client preferences are used in appraisal.
- Whether quick placement was considered in appraisal.
- Whether motivation was considered in appraisal.
- How well the appraisal process was integrated into WIN.
- Whether the site maintains an unassigned participant pool.
- Whether stop status impedes the progress of participants.
- The degree to which follow-up is used after placement.
- Degree to which the recipient is automatically terminated from WIN at WIN's request.
- General value of orientation.
- Nature of counseling provided.
- Nature of training available.
- Extent of training available.
- The extent to which CETA was used by WIN in 1974 and 1975.
- The availability of Adult Basic Education.
- The availability of WIN/OJTs (on-the-job training).
- The availability of WIN Public Service Employment.
- Nature and extent of WIN Job Search.

- Degree to which the WIN tax incentive is employed.
- Quality of the WIN placement service.
- Degree to which job development was used.
- Degree to which retroactive placement was used.
- Accessibility of WIN and welfare files.
- Overall estimation of staff quality — WIN and welfare.
- Quality of Manpower resources available to and used by program.
- Whether there were major budget changes from 1974 to 1975, and whether these affected WIN operations, including SAU activities.
- Nature of child care available through the SAU.
- Degree to which child care is incorporated into employability plan.
- Whether the SAU staff were specialists or generalists.
- Degree of physical integration between WIN and SAU staffs.
- Degree of functional integration between WIN and SAU staffs.
- Quality of community resources available to and used by WIN.

F. 2 Observed Significance

Each of these characteristics had two or more levels, and each was related to the mean earnings changes of both participants and comparison-group respondents at that (site) level. These relationships were then examined to identify significant variations in (1) participant earnings changes, (2) comparison-group earnings changes, or (3) the difference between (1) and (2). This procedure enabled us to determine whether "net" changes were generated more by participant experiences or comparison-group experiences. Of the variables examined in this way, only the following showed promise:

- How appraisal control is exercised in combination with the degree to which employability was used as a screening variable.
- Whether the site maintained an unassigned participant pool.
- If stop status impeded progress of participants.
- The extent to which counseling was used.
- The extent of training available.
- The degree to which WIN, OJT, and PSE was available.
- Nature of the job search component.
- The degree to which retroactive placement was used at the site.
- The availability of manpower resources at the site.

These were chosen because of the strong monotonic relationship between the variable categories and participant earnings, and a reasonable relationship between the category and the difference between the participants and comparison groups.*

These particular site characteristics were introduced into our basic regression model in order to determine whether or not they appeared to exert any independent influence of WIN impact. In general, they added nothing to the explanatory power of our models. At most, it appeared that the availability and utilization of community manpower sources had a significant effect on program outcomes, but no further analysis of this or the other program variables was undertaken.

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* The relationship to participant earnings rather than just to "net" differences was considered important since only these should have been affected, as mentioned in the text.

The apparently weak influence of local program characteristics on WIN impact probably reflects more than the shortcomings of our methodology.* First of all, site variables do not necessarily affect every participant at a site. Unlike the service levels, a participant at a given site may have been exposed to a program having extensive training, but not have received training himself. Thus, the relationship would be intrinsically weaker for an overall site variable than for a participant-attached service variable. Second, the variation in program components is rather restricted in the WIN-II concept. The range between the limits of many variables (e.g., none at all, little, occasional, extensively used) actually defines a fairly narrow range.

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* As noted earlier, computer constraints precluded a more detailed methodological exploration.

Appendix G: ADDITIONAL TABLES

The appendix contains additional material for various sections of the main text, as cited therein. For convenience, it may be noted that the following are included:

G.1: Changes in Area Unemployment Rates Spring 1974 - Spring 1975

G.2: Major Combinations of Employment Services for Females, by UF, non-UF, and Rural Sites

G.3: WIN Supportive Service Received by Male Clients

G.4: WIN Supportive Service Received by Female Clients

G.5 Reasons for Leaving Welfare, by Race and Sex

Table G.1 Changes in Area Unemployment Rates, Spring 1974 - Spring 1975, by WIN Site

	UNEMPLOYMENT RATE			
SITE NAME	1974	1975	ABSOLUTE CHANGE	RELATIVE INCREASE
Philadelphia	6.1%	9.8%	3.7%	61%
Hyattsville (Prince Georges Co.)	3.9	4.9	1.0	26
Pittsburgh	7.6	8.3	0.7	9.2
Waynesburgh	3.8	6.8	3.0	79
New Castle	6.2	11.5	5.3	85
Fayetteville*	1.7	6.7	5.0	294
Midland *	9.7	11.0	2.3	24
Helena* (Montana)	6.7	8.6	1.9	28
Sonoma Co.	9.9	13.5	3.6	36
Macon	4.6	7.7	3.1	67
Montgomery (3 county area)	3.3	6.6	3.3	100
Selma	8.1	9.7	1.6	20
Bemidji	7.1	6.8	-0.3	-4.0
Duluth	7.3	7.7	0.4	5.0
Mobile*	5.2	7.2	2.0	38
Culver City	6.3	9.9	3.6	57
Inglewood	5.8	9.2	3.4	59
Syracuse *	4.7	9.7	5.0	106
Atlantic City *	5.7	8.7	3.0	53

SITE NAME	UNEMPLOYMENT RATE 1974	UNEMPLOYMENT RATE 1975	ABSOLUTE CHANGE	RELATIVE CHANGE***
Duarte (San Gabriel Valley)	6.6%**	10.1%	3.5%	53%
E. Los Angeles (Central Co.)	6.6**	9.8	3.2	48
Detroit A5	9.3	16.2	6.9	74
San Fernando* (L.A. City)	7.4**	11.1	3.7	50
Van Nuys* (L.A. City)	7.4**	11.1	3.7	50
St. Louis	5.8	9.4	4.4	62
E. St. Louis	6.7	10.3	3.6	54
Pasadena	5.0	7.9	2.9	58
Visalia	9.3	12.6	3.3	35
Cincinnati	6.2	8.0	1.8	29
Delray Beach	7.1	14.8	7.7	108
Alexandria (Baton Rouge)	9.0	11.7	2.7	30
Albuquerque	6.4	8.4	2.0	31
Wayne Co. (Huntington)	6.6	7.8	1.2	18
Sanford	3.5	6.6	3.1	89
Greenwood*	3.4	6.7	3.3	97
Las Cruces*	6.5	9.4	2.9	45
Jamaica Queens	5.7	9.6	3.9	68

	UNEMPLOYMENT RATE			
SITE NAME	1974	1975	ABSOLUTE CHANGE	RELATIVE CHANGE***
McAllen	9.0%	9.4%	0.4%	4.0%
Ashtabula *	4.3	10.1	5.8	135
West Chester*	5.7	8.9	3.2	56
Vincennes	6.1	12.5	6.4	105
Houston *	3.6	4.6	1.0	28
Kansas City	3.0	7.8	4.8	160
Columbia	5.3	12.7	7.4	140
Peoria	3.2	4.5	1.3	41
Walterboro (Charleston)	4.7	13.3	8.6	183
Georgetown (Charlestown)	4.7	11.2	6.5	138
Waterloo *	5.6	5.6	0.0	0.0
Norwich (Hartford)	6.7	10.9	4.2	63
Norristown	6.8	12.7	5.9	87
Boulder (Denver Metro)	4.0	6.1	2.1	53
Wilkes Barre	7.5	12.1	4.6	61
Pontiac * (Detroit SMSA)	9.3	16.2	6.9	74
Portsmouth*	4.7	7.1	2.4	51
Ft. Morgan (Ft. Collins)	4.0	4.3	0.3	8.0
Cheyenne (Wyoming)	3.2	4.7	1.5	47

	UNEMPLOYMENT RATE			
SITE NAME	1974	1975	ABSOLUTE CHANGE	RELATIVE CHANGE***
Nashville*	3.3%	7.5%	4.1%	127%
Cookeville	6.0	13.4	7.4	123
San Francisco	8.2	11.4	3.2	39
Hayward (San Francisco)	8.3	11.6	3.3	40
Humboldt * (2 counties)	7.1	8.7	1.6	23
Portland	6.3	9.6	3.3	52
Jamaica Pln.	7.5	11.5	4.0	53
Quincy (Boston SMSA)	7.5	11.5	4.0	53
Spencer	7.3	15.4	8.2	111
Sacramento	7.9	9.6	1.7	22
Lancaster *	2.7	7.3	4.6	170
Sheboygan	3.6	7.4	3.8	106
Muskegon *	11.3	14.9	3.6	32
Fort Worth *	3.1	5.1	2.0	65
Chicago	4.6	8.9	4.5	93
Michigan City	4.1	9.6	5.5	134
Tulsa	3.6	4.9	1.3	36
Opelika*	2.7	7.8	5.1	189
Roseburg	8.6	14.2	5.6	65
Bronx	7.0	11.8	4.8	69

	UNEMPLOYMENT RATE			
SITE NAME	1974	1975	ABSOLUTE CHANGE	RELATIVE CHANGE***
Manhattan	7.8%	13.0%	5.2%	67%
Detroit* (Greenfield)	9.3	16.2	6.9	74

* part of larger SMSA
** rate from Area Trends — part of SMSA for specific entry

NOTE: Unemployment rates cited herein are, unless otherwise noted, rates computed for purpose of CETA fundings. CETA areas do not in all cases correspond to the established SMSA. To make this distinction, those sites identified by CETA as distinct CETA areas are simply noted by the site name. Where CETA could not isolate the rate for a specific site area (Los Angeles sites, for instance), the unemployment rate for the corresponding SMSA is used, and those sites are identified with an asterisk. Rates which include larger areas than just the site, e.g., 3 county areas, or an entire state (Montana), are noted as such. Whenever possible, the smallest area of coverage was used.

G.2 Major Combinations of Employment Services for Females by UF, Non-UF, and Rural Sites

	UF	Non-UF	Rural
No Employment Services	6.5%	6.5%	9.3%
Initial Services Only*	15.5	12.5	16.8
Initial Services Plus Education or Training	8.7	9.9	8.9
Initial Services Plus Job Development or Placement	35.5	35.7	33.0
All Services	20.1	23.2	19.0
Other and Unknown	13.7	12.2	13.0
	100 %	100 %	100 %
Number	1224	751	321

* includes appraisal, orientation, or employability planning

Table G.3 WIN SUPPORTIVE SERVICE RECEIVED BY MALE CLIENTS

	MEDICAL CARD	CHILD CARE	FOOD STAMP	PUBLIC HOUSING	TRANSPORTATION
PARTICIPANTS					
Some Yes	707	66	622	184	67
No	77	723	162	588	724
Unknown	6	1	6	18	2
COMPARISONS					
Some Yes	426	4	348	120	5
No	91	514	167	375	515
Unknown	1	0	3	23	1

Table G.4 WIN SUPPORTIVE SERVICE RECEIVED BY FEMALE CLIENTS

PARTICIPANTS					
Some Yes	2415	959	2120	811	953
No	114	1562	410	1676	1563
Unknown	7	15	6	49	16
COMPARISONS					
Some Yes	1347	94	1161	484	94
No	104	1358	289	936	1359
Unknown	3	2	4	34	3

Table G. 5 Reasons for Leaving Welfare, by Race and Sex

	MALES		FEMALES	
	White	Black	White	Black
Percent who left welfare	50.2%	31.8%	30.2%	19.5%
Reason for Leaving:				
Got job	70.4%	52.0%	48.6%	59.6%
Got married	0.3%	1.3%	8.8%	6.2%
Spouse returned	0.7%	0.0%	6.6%	4.7%
Other benefits	5.6%	6.7%	6.6%	5.8%
Grant cut off	8.6%	10.7%	13.8%	13.8%
Other	15.4%	33.3%	16.7%	13.1%
Total	100 % (267)	100 % (75)	100 % (319)	100 % (275)

Source: Wave III interviews

R.M. Valid ____________
PTTA Valid ____________

PACIFIC T. & T.A. CORP.

O.M.B. No. 44-S75013
Approval Expires March, 1976
R.M. 10% Quality Control ____________
PTTA 10% Quality Control ____________

CARD TYPE = 25 (6, 7)

ENTER CURRENT NAME, ADDRESS AND PHONE NUMBER AT TIME OF INTERVIEW OR FINAL ATTEMPT:

NAME 8 9 10 11 12 13 14 15 16 17 First | 18 Middle I. | 19 20 21 22 23 24 25 26 27 28 29 30 Last

ADDRESS 31 32 33 34 35 36 37 38 39 40 41 42 43 44 45 46 47 48 49 50 Street and Number

51 52 53 54 55 56 57 58 59 60 61 62 City | 63 64 State | 65 66 67 68 69 Zip Code

PHONE 70 71 72 Area Code | 73 74 75 - 76 77 78 79 Number

CONTACT HISTORY

ATTEMPT NUMBER	MONTH	DAY	TIME	CONTACT METHOD & RESULTS	INITIALS
1					
2					
3					
4					
5					
6					
7					

FINAL RESULT

REPEAT IDNL

CARD TYPE = 26 (6, 7)

CODES

1. Not Home
2. Refused
3. Moved Out of City; verified by: ____________
4. Died; verified by: ____________
5. Armed Services; verified by: ____________
6. Jail; verified by: ____________
7. Hospital/Inst.; verified by: ____________
8. Completed Interview
9. Other(Specify); ____________

ENTER CODES

Code Final Attempt Number 8 ______
Code Month of Final Attempt 9-10 ______
Code Day of Final Attempt 11-12 ______
Code Final Result 13 ______

Other Contacts
(1) ____________
(2) ____________

I certify that the above information is true and accurate to the best of my knowledge.

____________ (Signature of Interviewer)

____________ Date

In speaking with you this time we would like to learn what changes have taken place in your family and in your experiences with employment and social services since we last spoke with you.

SECTION I: SCREENING INFORMATION

______ 14 1. Have you ever participated in the WIN Program? PROBE CAREFULLY

1 Yes ↓ 2 No → Skip to Question 21, page 7 (White)

______ 15 1a. Are you now participating in the WIN program? PROBE CAREFULLY

1 Yes → Skip to Question 3 Page 2 (Yellow)

2 No ↓

______ 16 2. Have you participated in WIN at all since ______________ ? PROBE
(see date on sticker)

1 Yes ↓ 2 No → Skip to Question 21, Page 7 (White)

17-
___:___ 18 2a. How long ago did you leave WIN?

CODE ______________________ Weeks

______ 19 2b. Why did you leave WIN?

WRITE REASON AND THEN CODE

____

____

1 Got Job
2 Left Welfare
3 Dropped Out
4 Became Exempt
(Why______________________)
5 Got Sick
6 Became Pregnant
7 Just never heard from them again
8 Other

NOTE: CONTINUE TO NEXT PAGE

SECTION II: WIN PROGRAM EXPERIENCE

(For those who are now in WIN or have participated since last interview)

II.A Background Information

3. How long have you participated in WIN? (Current or most recent participation only) 20-21 ____

CODE ______________________ Weeks

*(4. What are you currently doing in WIN?) 22 ____

PROBE IF NECESSARY

1.	Orientation	5.	Skill Training--Classroom
2.	Personal Counseling	6.	Skill Training--On-the-job
3.	Job Search/Job Counseling	7.	Placed in a job
4.	Basic Education	8.	Nothing
		+	Other:

5. Did you volunteer for WIN? 23 ____

1 Yes 2 No → SKIP TO QUESTION 6

5a. Why? 24 ____

RECORD RESPONSE:

____

____

CODES

1. Would lose grant for failure to participate
2. Training Opportunity
3. Job Opportunity
4. Other

6. Did you attend a meeting with Welfare and WIN people to see whether you should participate in the program? (This meeting is sometimes called an appraisal interview) 25 ____

1 Yes 2 No

II.B Orientation

7. Have you ever attended any orientation classes? 26 ____

1 Yes 2 No → SKIP TO QUESTION 8 PAGE 3

*(7a. Are you still attending orientation classes?) 27 ____

1 Yes → SKIP TO QUESTION 7e PAGE 3

2 No

7b. Did you complete all of the orientation classes? 28 ____

1 Yes 2 No

7c. How long did you attend these classes? ____________ DAYS 29-30 ____

CODE ____________ HOURS PER DAY 31 ____

* ASK CURRENT PARTICIPANT ONLY

_______32 7d. Were (are) these classes helpful?

1 Yes 2 No

_______33 7e. While you were in orientation, did anyone from WIN help you look for a job?

1 Yes 2 No

II.C Employability Plan

_______34 8. Did you have a particular kind of job in mind when you first entered WIN?

1 Yes ↓ 2 No → SKIP TO QUESTION 9

_ _ _ 35-37 8a. What type of job did you want?

RECORD RESPONSE - PROBE: Type of work, duties

____

_______38 9. Has anyone in the WIN program ever talked to you about a plan or schedule for what you would do in WIN?

1 Yes 2 No

_______39 10. Are you satisfied with your activity in WIN?

1 Yes → SKIP TO QUESTION 11

2 No ↓

_______40 10a. Why not? RESPONSE: _______________________________

CODES:

1. Didn't get training
2. Didn't get a job
3. Didn't get the kind of job I wanted
4. Getting no services
5. Other

_______41 11. Did WIN give you any tests to determine your skills or interest?

1 Yes ↓ 2 No → SKIP TO QUESTION 12

_______42 11a. Did anyone from the WIN staff discuss the test results with you?

1 Yes 2 No

12. Have you received any incentive payments for participating in WIN?

1 Yes ↓ 2 No → DO NOT CODE BUT SKIP TO QUESTION 13

12a. For what purpose? 43 ______

1. $30 per month incentive
2. Transportation
3. Work related expenses
4. Other

12b. How much is that per week?________________ 44-45 ______

12c. From?______________(Month) 46-47 ______

To? ______________(Month) 48-49 ______

II.D Education (i.e. Basic Education) in WIN

13. Have you attended any education classes in WIN, for example GED? 50 ______

1 Yes ↓ 2 No → SKIP TO QUESTION 16, Page 5

14. While you were taking classes, did WIN help you look for a job? 51 ______

1 Yes 2 No

*(15. Are you still attending?) 52 ______

1 Yes → SKIP TO QUESTION 16

2 No ↓

15a. How many weeks did you attend?______________ 53-54 ______

15b. How many hours per week was that?______________ 55-56 ______

15c. Did you complete? 57 ______

1 Yes ↓ 2 No → SKIP TO QUESTION 15e.

15d. Did you get your diploma, GED or Certificate? 58 ______

1. Diploma
2. GED
3. Certificate
4. Other: ______________
5. Nothing

15e. Did you think the education was helpful? 59 ______

1 Yes 2 No

II.E Skill Training in WIN

______ 60 16. Have you attended any vocational training classes in WIN?

1 Yes ↓ 2 No → SKIP TO QUESTION 17

__ : __ : __ 61-63 16a. What type of training did you receive?

RECORD RESPONSE:

____

______ 64 16b. Do you think the training was helpful?

1 Yes 2 No

______ 65 16c. when you were in training, did anyone from WIN help you look for a job?

1 Yes 2 No

______ 66 *(16d. Are you still attending?)

1 Yes → SKIP TO QUESTION 17

2 No ↓

__ : __ 67-68 16e. How many weeks were you in training? ______________

__ : __ 69-70 16f. How many hours per week was that? ______________

______ 71 16g. Did you complete the training?

1 Yes ↓ 2 No → SKIP TO QUESTION 17

______ 72 16h. Did you receive a diploma or certificate?

1. Diploma
2. Certificate
3. Other: ______________
4. Nothing

II-F Job Development and Placement

______ 73 17. Has anyone from WIN or the State Employment Office talked to you about specific job openings?

1 Yes 2 No

______ 74 18. Did they contact any employers for you?

1 Yes

2 No

9 Don't Know

9. Were you sent out on any job interviews? 75 ______

1 Yes 2 No → SKIP TO QUESTION 20

19a. How many times? ______________ 76-77 __|__

19b. Were you interested in any of these jobs? 78 ______

1 Yes → SKIP TO QUESTION 20

2 No ↓

19c. Why not? 79 ______

1. Low wages
2. Too far away
3. Physically unable to do the work
4. Other

0. Did WIN ever place you in a job? 80 ______

1 Yes ↓ 2 No → SKIP TO QUESTION 21, Page 7

REPEAT COLS 1-5

CARD 2|7

8-10 __|__|__

20a. What kind of job was it?

IF MORE THAN ONE GET MOST RECENT. RECORD RESPONSE - PROBE: "Type of work, duties"

____

20b. Do you know if the job was -- READ ALL 11 ______

CODE

1. On the Job Training (OJT)
2. Public Service Employment (PSE or PEP)
3. Regular job with private employer?
9. Don't know

20c. Are you still working on this job? 12 ______

1 Yes → SKIP TO QUESTION 21 (White Section)

2 No ↓

20d. Why did you leave that job? PROBE 13 ______

1. Fired
2. Quit
3. Laid Off
4. Project was completed
5. Other: ______________

____

III. SUPPORTIVE SERVICES

Now I would like to ask some questions about things that may influence your employment:

21. Do you have any medical problems that could keep you from working, or limit the type or amount of work you could do? 14 ______

1 Yes 2 No → SKIP TO QUESTION 22

21a. What are these problems? RECORD RESPONSE ______________________ 15 ______

PROBE, IF NECESSARY TO DETERMINE CODE:

e.g.
"Can you do any work at all?"
"How long will this condition last?"

1. Total Permanent disability
2. Total Temporary disability
3. Partial Permanent disability
4. Partial Temporary disability

22. Does anyone else in your home have a medical problem so severe that you must spend large amounts of time caring for them? 16 ______

1 Yes 2 No → SKIP TO QUESTION 23

22a. Would this prevent you from taking a job? 17 ______

1 Yes 2 No

23. Roughly, about how many times did you and your family have to go to a doctor, clinic, or hospital for treatment during the last six months? 18-19 ___|___

______________________ CODE

24. In general, do you use your medical card (Medicaid) to pay for such treatment? 20 ______

1 Yes → SKIP TO QUESTION 25

2 No

24a. Why not? RECORD RESPONSE: ______________________ 21 ______

CODE

1. Don't have a card
2. Have other medical coverage
3. Doctors won't accept it
4. Weren't sick
5. Other

25. Do you have any children (i.e. in the household and under age 15)? 22 ______

1 Yes 2 No → SKIP TO QUESTION 29

CONTINUE TO NEXT PAGE

_______ 23 26. Do you have to arrange for someone else to provide day care or sit for your children if you are attending classes, looking for work or at work?

1 Yes → SKIP TO QUESTION28, CHILD CARE MATRIX

2 No ↓

_______ 24 27. Would you have difficulty making arrangements if you had to be away from home all day?

1 Yes ↓   2 No → SKIP TO QUESTION 29

_______ 25 27a. What difficulty? CODE

1. Finding someone to care for children
2. Cost
3. Financing quality care
4. Transportation to sitter
5. Other: ____________________

SKIP TO QUESTION 29

CHILD CARE MATRIX

28. What are the ages of your children under 15, beginning with the youngest? (ENTER ALL AGES IN MATRIX)

28a. Who takes care of the (age) -year old?

CODES

1. Spouse
2. Older children

CODE & SKIP TO QUESTION 28e

3. Baby-sitter: resident of household
4. Baby-sitter: Non-resident friend or relative
5. Baby-sitter
6. Friend or relative's home
7. Family day care
8. Day care center (nursery)
9. Cares for self (e.g., after school)
10. Other:____________

28b. Did anyone from welfare or WIN help you in making this arrangement?

1 Yes   2 No

28c. How much do you pay per week for these arrangements (Out of your own pocket)?

RECORD AMOUNT PER CHILD: IF NOTHING, LEAVE BLANK

28d. Does someone else (WIN, welfare, a friend, etc) pay anything for these arrangements?

CODE

1. Welfare
2. WIN
3. Relative
4. Friend
5. Other
6. No one else pays

28e. Are you satisfied with the day care arrangements you have now? CODE IN MATRIX

1. Yes
2. No--Transportation problem to day care
3. No--Hours care is available
4. No--Where center is located
5. No--Quality of care PROBE & RECORD
6. No--Cost
7. No--Other: RECORD

RETURN TO QUESTION 28a FOR NEXT CHILD

CHILD CARE MATRIX

CO1	28 AGE	28a WHO TAKES CARE (CODE)	28b DID WIN/ WELFARE HELP 1=Yes 2=No	28c OUT OF POCKET COST PER WEEK	28d SOMEONE ELSE PAY (CODE)	28e (CODE)	SATISFIED WITH CARE? RECORD RESPONSE
26							
35							
44							
53							
62							
71							

ENTER TOTAL NUMBER OF CHILDREN UNDER 15 NOT ACCOUNTED FOR IN THE TABLE ____________ 80 ________

REPEAT COLS 1-5 CARD 2 8

29. Have you attended any vocational training classes since the last time we interviewed you, other than those sponsored by WIN?

1 Yes 2 No → SKIP TO QUESTION 31 8 ________

30. What type of training was it? 9-11 ________

RECORD RESPONSE: (e.g., machine set-up operator)

____

30a. How long were you in this training? 12-13 ________

CODE: ____________________ Weeks

30b. How many hours per week is that? 14-15 ________

CODE: ____________________ Hours

30c. Did you complete the training? 16 ________

1 Yes 2 No → SKIP TO QUESTION 30e

30d. Did you receive a diploma or certificate? 17 ________

1. Diploma
2. Certificate
3. Other:
4. Nothing

30e. What organization or agency provided the training? 18 ________

READ IF NECESSARY

1. Voc. Rehabilitation
2. CETA
3. OIC
4. Private employer
5. Other Manpower
6. School System
7. Other: ____________
9. Don't Know

19 BLANK

________ 20 31. Do you have a car?

1 Yes 2 No

________ 21 32. Do you have transportation problems getting to places such as jobs, training, welfare, WIN offices?

1 Yes 2 No

________ 22 33. Are you currently buying food stamps?

1 Yes 2 No → SKIP TO QUESTION 34

↓

__|__|__ 23-25 33a. How much do you pay for these stamps?

CODE: ________________ Dollars

__|__|__ 26-28 33b. What is the (face) value of these food stamps (at retail)?

CODE: ________________ Dollars

________ 29 33c. How often do you buy food stamps?

CODE: Every ________________ Weeks

________ 30 34. Do you live in public housing?

1 Yes 2 No

SECTION IV: WORK/WELFARE HISTORY

________ 31 35. Are you or any members of your family now receiving AFDC Welfare benefits?

1 Yes → SKIP TO QUESTION 36

2 No

↓

__|__ 32-33 35a. When did you stop receiving AFDC? RECORD Month ________

__|__ 34-35 Year ________

________ 36 35b. What circumstances enabled you to leave AFDC?

Record Response ________________________________

CODES
1. Got Job
2. Got married
3. Spouse returned
4. Received other benefits (Soc Sec, VA, etc.)
5. Cut off my grant
6. Other: ____________

________ 37 36. Have you had any contact with welfare or WIN since ____________?
(see date on sticker)

1 Yes 2 No → SKIP TO P.12, WORK/WELFARE CALENDAR

↓

CONTINUE TO TOP OF NEXT PAGE

37. Has Welfare or WIN provided any transportation allowance for you? 38 ____

1 Yes 2 No → SKIP TO QUESTION 38

37a. How much money per week is that? 39-40 ____

RECORD: ____________ Dollars

37b. How many weeks have you received this allowance? ____________ 41-42 ____

37c. Has this solved your transportation need? 43 ____

1 Yes → SKIP TO QUESTION 38

2 No

37d. Why not? CODE 44 ____

RESPONSE: ____________

____

1. It's not enough money
2. Public transportation is inadequate
3. Car troubles
4. Jobs or WIN are too far away
5. Other

38. Welfare and WIN offices have counselors or advisors who talk to people about plans, jobs or personal problems. Have you met with one of these people since we last interviewed you? 45 ____

1 Yes 2 No → SKIP TO QUESTION 39 BELOW

38a. How many times? ____________ CODE 46 ____

38b. Did you find it helpful? 47 ____

1 Yes 2 No

39. Have you felt that you needed any services from Welfare or WIN that it did not provide you? 48 ____

1 Yes 2 No → SKIP TO QUESTION 40 BELOW

39a. Could you describe them? 49-50 ____

RECORD RESPONSE
THEN ENTER CODE ____________

____

40. Of all the services or help Welfare/WIN has given you which service was the most helpful? 51-52 ____

CODE

CODES

1. Day Care
2. Transportation
3. Medical
4. Education
5. Training
6. Job Placement
7. Family Planning
8. Housing Improvement
99. Don't Know
10. Financial Aid
11. Counseling
12 None
13. Other: ____________

____

QUESTIONS FOR WORK/WELFARE MATRIX

41. Were you employed at anytime since December 1, 1974?

1 Yes 2 No → LEAVE COLS 1-5 BLANK AND SKIP TO QUESTION 46

↓

IF YES, CHECK NUMBER OF WEEKS WORKED IN EACH OF THE APPROPRIATE MONTHS

42. What kind(s) of job(s) did you hold?

ENTER JOB DESCRIPTION(S) IN COLUMN (2)

43. Did WIN help you in getting this job? ASK FOR EACH JOB AND CODE IN COLUMN 3

1 Yes 2 No

44. How much were you paid per week (in gross wages, i.e. before payroll deductions)?

RECORD WAGES (ROUNDED TO NEAREST DOLLAR) IN COLUMN (4). NOTE WAGE INCREASES FROM MONTH TO MONTH.

45. How may hours per week did you work at this job?

RECORD HOURS PER WEEK IN COLUMN (5) AS ROUNDED TO THE NEAREST HOURS FOR APPROPRIATE MONTHS FOR ALL JOBS.

46. During the times when you were not working, were you looking for work?

PROBE TO MAKE SURE, BY ASKING: WHAT WERE YOU DOING TO LOOK FOR WORK? HOW OFTEN DID YOU DO THIS?

IF YES, CHECK NUMBER OF WEEKS SPENT LOOKING IN EACH MONTH RESPONDENT WAS UNEMPLOYED (COLUMN 6)

47. What were you doing at those times when you were neither working nor looking for work?

CODE FOR EACH MONTH IN COLUMN (7)

48. Have you or your family been receiving AFDC benefits each month?

1 Yes 2 No

CODE FOR EACH MONTH IN COLUMN (8)

NOTE: If respondent has not worked since Dec. 1st complete columns 6-8 in matrix, then skip to CURRENT JOB SEARCH, Page 16.

NO.	C O L	(1) WORK, Weeks per month: -none 1 one 2 two 3 three 4 four + indefinite	(2) PRIMARY OCCUPATION		(3) WIN JOB? 1 = Yes 2 = No	(4) WEEKLY WAGES ($'s)	(5) HOURS/ WEEK	(6) SEEKING WORK? Weeks per month - no 1 one 2 two 3 three 4 four	(7) OTHER DATA 1. Houskeeping 2. WIN 3. School 4. Training 5. Institution 6. Armed Forces 7. Serious Illness 8. On Strike + Vacation/ off season Other	(8) AFDC WELFARE 1. Yes 2. No
75 ct .	63									

Repeat Cols 1-5 Card 29

p	11									
	24									
	37									
	50									
	63									

Repeat Cols 1-5 Card 30

	11									
	24									
	37									
	50									
74	63									

NOTE: If respondent was not employed since December 1, 1974, skip to "Current Job Search," p. 16.

Otherwise ask:

______ 76

REPEAT COLS
1-5
CARD 3 1

49. Did you hold more than one job at any one time since December 1, 1974?

1 Yes ↓ 2 No → SKIP TO QUESTION 50 BELOW

COL 8-23

______
Occupation

Code	Hours/ week	Weekly wages	Mo yr from	Mo yr to

NOTE: If employed at present → SKIP TO QUESTION 51 BELOW

If employed since December 1, 1974, but not now, Ask Question 50:

______ 24

50. Why did you leave that job (last job)?

RECORD RESPONSE: ______

FIRST PROBE FOR CLARITY: THEN CODE REASON CLOSEST TO RESPONSE GIVEN ABOVE

1. Fired
2. Laid off or project ended
3. Quit - didn't like work
4. Quit - low wages
5. Quit - to care for children
6. Quit - got sick
7. Quit - got married
8. Pregnancy/Childbirth
- Quit - Moved
+ Other:

NOTE: SKIP TO IV-D "CURRENT JOB SEARCH" QUESTION 55, p. 16

IV-C. Present Job

25-27

51. What type of business or industry do you work for?

RECORD RESPONSE, e.g., "retail food store, steel mill, etc.

______

51a. Were you employed at this same job when we last interviewed you? 28 ______

1 Yes → SKIP TO QUESTION 53

2 No ↓

52. Do you work for a private employer, the government or are you self-employed? 29 ______

1. Private employer	3. Self-employed
2. Government	4. Other: ____________________

53. How did you hear about your present job? 30 ______

CODE

1. Public Employment Service	5. Friends/Relatives
2. Union	6. Private Employment Agency
3. WIN	7. Radio/TV/Want Ads
4. Employer directly	8. Other: ______________

54. Has anyone from WIN contacted you since you got this job? 31 ______

1 Yes ↓ 2 No → SKIP TO QUESTION 55 "CURRENT JOB SEARCH"

54a. How were you contacted? 32 ______

1. Mail	3. Visit on job
2. Phone	4. Visit at home

54b. What kinds of things did they ask you? 33 ______

RECORD RESPONSE: ______________________________

CODE CLOSEST RESPONSE

1. Whether I was working
2. How the job was working out
3. Whether I needed any further help or assistance
4. Other:

IV-D Current Job Search

34 ______ 55. Are you now looking for work?

1 Yes → SKIP TO QUESTION 56 BELOW

2 No ↓

35 ______ 55a. What reasons are there for your not looking for work?

RECORD RESPONSE ____________________

CODE REASON CLOSEST TO RESPONSE GIVEN ABOVE

1. Child care
2. Employed
3. Illness
4. Full-time school
5. Age
6. Lack of skill/experience
7. Lack of Transportation
8. Pregnancy

- No jobs available

+ Other:

SKIP TO SECTION V., p. 17 →

(ASK QUESTIONS 56 AND 56a FOR EACH SOURCE LISTED BELOW)

56. In looking for work, do you use (Source) * ? CODE EACH ONE

56a. Did you use it last week?

CODE: 1 Yes 2 No

	*Source	Ever Use		Use Last Two Weeks	
		YES	NO	YES	NO
36	1. State Employment Office	1	2	1	2
38	2. WIN	1	2	1	2
40	3. Friends/Relatives	1	2	1	2
42	4. Want Ads/Radio/TV	1	2	1	2
44	5. Private Employment Agencies	1	2	1	2
46	6. Unions	1	2	1	2
48	7. Employers Directly	1	2	1	2
50	8. Other: ____________	1	2	1	2

52 ______ 56b. In your search for jobs, what seems to be your most difficult problem?

1. Transportation
2. Lack of training experience
3. Adequate day care
4. No jobs available
5. Don't know where jobs are
6. Age
7. Discrimination
8. Other: ____________

SECTION V: PERSONAL DATA

READ: "Now I would like to ask you some questions about yourself and your family."

57. What month, and year were you born?

RECORD: Month________________ 53-54 ___|___

Year________________ 55-56 ___|___

58. Are you now... READ ALL RESPONSES 57 ______

1. Married
2. Separated
3. Divorced
4. Widowed
5. Never Married

QUESTIONS FOR HOUSEHOLD MATRIX

59. READ: "Now I would like to ask you about the children, relatives and friends living in your household at the present time:

59a. What is the date of birth(month&year) of the youngest household member? ENTER IN MATRIX

59b. What is that person's relationship to you?

WRITE RELATIONSHIP (E.G., SON, FATHER, SISTER-IN-LAW, FRIEND, ETC. THEN ENTER CODE IN MATRIX)

59c. **IF NOT OBVIOUS, ASK:** Is that person male or female?

ENTER CODE

59d. What is this person now doing?

1. In school
2. In school and working
3. Working full-time
4. Working part-time
5. Looking for work
6. Housekeeping/at home
7. Seriously ill
8. Training

- Training and working

+ Other

CODE CLOSEST ACTIVITY IN MATRIX

60. FOLLOW SEQUENCE OF QUESTIONS 59a - 59d for each household member

HOUSEHOLD COMPOSITION MATRIX

COL	RELATIONSHIP 1. Own child 2. Other child 3. Spouse 4 Parent 5. Other Relative 6. Friend (Write relationship)	CODE	DATE OF BIRTH Month	Year	SEX 1. Male 2. Female	CURRENT ACTIVITY 1. In school 2. In school and working 3. Working full-time 4. Working part-time 5. Looking for work CODE	6. At Home/ Housekeeping 7. Seriously ill 8. Training - Training and working + Other
**	Repeat Cols 1-5 Card 32						
8							
15							
22							
29							
36							
43							
50							
57							
64							
71							
	Repeat Cols 1-5 Card 33						
8							

ENTER TOTAL NUMBER IN HOUSEHOLD INCLUDING RESPONDENT → 15-16 ______

17-19 61. Counting your family income from all sources how much money did you receive last month?________________(Note: Ask GROSS income, before deductions.)

20-21 61a. How many persons does that income provide for?________________

22-24 61b. Of that amount how much was from your own employment?____________

25-27 61c. How much was from the employment of other people in the family?

____

61d. Did you or any other household members receive any income from the following sources:

CODES: 1 Yes 2 No

		RESPONDENT		OTHER MEMBERS	
28	Pension	☐1	☐2	☐1	☐2
30	Social Security	☐1	☐2	☐1	☐2
32	Supplemental Security Income (SSI)	☐1	☐2	☐1	☐2
34	General Assistance Welfare	☐1	☐2	☐1	☐2
36	Alimony or child support	☐1	☐2	☐1	☐2
38	Unemployment Compensation	☐1	☐2	☐1	☐2
40	Workman's Compensation	☐1	☐2	☐1	☐2
42	Veteran's Benefits	☐1	☐2	☐1	☐2
44	Other (Specify)____________________	☐1	☐2	☐1	☐2

---

46 62. Was your family income last month typical of what you have received during the last six months?

1 Yes → SKIP TO QUESTION 63

2 No ↓

47-49 62a. What has your typical monthly income been during the last six months?____________________________

50 62b. What accounts for the change in income? RECORD ________________

CODE CLOSEST REASON

1. Respondent got or lost job
2. Other family members got or lost job
3. Someone moved in or out of household
4. Increase or decrease in welfare grant
5. Raise in salary
6. Other: ________________

51-52 62c. What was the date that the change took place? ________ Month

53-54 ________ Year

		CARD
63. Are there any additional comments about the things we have been talking about that you would like to make?		55 ____

RECORD RESPONSE:

CODE: 1=Comment
9=No Comment

INTERVIEWER OBSERVATIONS

Were other people present during the interview?	1 Yes 2 No	56 ____
Did the respondent have a language difficulty because English is not his native language?	1 Yes 2 No	57 ____

List the Questions which tended to cause the most difficulty for the respondent to answer. 58-60 ____ ____

____

____

____

List any questions which the respondent refused to answer. 62-64 ____ ____

____

____

____

List any questions which elicited a hostile response. 66-68 ____ ____

____

____

____

NOTE: BEFORE LEAVING THE RESPONDENT, PLEASE CHECK TO SEE IF ALL QUESTIONS HAVE BEEN ANSWERED. TOTAL LENGTH OF INTERVIEW ____ MINUTES. 70-72 ____